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## ORIGINAL ARTICLES.

### A METHOD OF APPLYING ANTISEPSIS IN THE TREATMENT OF RECENT ANTERIOR URETHRITIS.[1]


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THE application of the principle of antisepsis to the treatment of urethritis may be made for one of two reasons, and in one of two ways. The drugs employed may be given on account of their supposed action upon either the microbes of suppuration, or upon the alleged specific microbe of gonorrhœa—the gonococcus of Neisser.

They may be used locally, or may be administered by the mouth, although, even when given in that way, their action is largely a local one.

As to the *reason* for their employment, in the light of modern pathology it is not necessary to believe in an invariable and essential relation between the gonococcus and gonorrhœa to see a clear indication for the use of germicidal agents. The successful treatment of suppuration anywhere demands the employment of some such agent, though it must be admitted that when the pus is furnished by a mucous surface the difficulties of treatment—*i. e.*, of successful antisepsis—are greater than under most other circumstances; and it must likewise be admitted that those difficulties are much increased when the suppurative process is established in a canal like the male urethra, the anatomical peculiarities of which favor so markedly microbic growth or multiplication.

Its length and narrowness; the approximation of its walls during the intervals of urination; the delicate and actively absorbent mucous membrane which lines it; the passage through it at short intervals of an animal fluid of more or less irritant properties, and prone to decomposition; the opportunities offered by the various follicles and lacunæ, and by the curves and dilatations of the normal canal for the retention of any foreign matter, all render the problem of antisepsis a complicated and a difficult one.

While, as I have said, the employment of antiseptics in urethritis, or a belief in their efficacy does not necessitate a corresponding belief in the gonococcus as the sole cause of gonorrhœa, yet a consideration of the rationale of such treatment undoubtedly involves an inquiry into the natural history of that microbe, which is certainly present, whether as cause or effect, in the large majority of cases of acute gonorrhœa. Unfortunately, the results of culture experiments and of the influence of parasiticides upon gonococci external to the body do not afford satisfactory evidence as to the effect of the same agents when applied to the same germs embedded in the depths of an inflamed urethral mucous membrane. Bumm's investigations appear to show that, at first, they multiply by preference in the papillary layer, and find their way to the surface only during the latter part of the purulent, and during the subsiding (or muco-purulent) stage, and Finger uses this as a theoretical argument against the early administration of either astringents or antiseptics. Enough is known, however, to enable us to state with certainty that the agents which are effectual against the microbes of suppuration are not equally destructive of the gonococci, several competent observers having noted the fact that antiseptic solutions, when applied to gonorrhœal secretions outside of the body, have seemed to exert but little effect upon the gonococcus even when strong enough to destroy pus-cells almost immediately. The study of the question, as a problem in bacteriology, is not, however, the object of this paper.

The definite etiological relation between the gonococcus and gonorrhœa, although asserted by a large and an increasing number of respectable authorities, cannot yet be considered as absolutely demonstrated. The statement that the disease has been produced by inoculation of pure cultures of the gonococcus (the only scientific and demonstrative proof of such relation) rests, so far as I know, upon experiments which are numerically insufficient to entitle it to an unhesitating acceptance, though I am willing to admit that, after having always taught and contended for the non-specificity of gonorrhœa, I feel the weight of evidence to be gradually accumulating upon the other side of the question.

Clinically, however, we may now assume that, in the majority of cases of acute urethritis, gonococci, the microbes of suppuration, and other bacteria will be found intermingled, and that successful treatment by germicidal agents will involve the destruction of all the different varieties present. Dismissing, therefore, any further theoretical considerations, we may inquire what drugs, if any, belonging to the class of antiseptics have a curative effect upon the

[1] Read before the American Association of Andrology and Syphilology, at Altoona, June 4, 1890.

essential symptom of urethritis—the blennorrhœa; restricting our inquiry to those cases in which the disease is comparatively acute, and in which it is limited to the pendulous urethra; in other words, to the ordinary cases of uncomplicated gonorrhœa which present themselves in one's office for treatment during the first few days after its appearance. The symptoms, pathology, complications, and treatment of the so-called "posterior" urethritis are entirely different, and need not be discussed in this relation. Considering topical remedies first, and excluding a large number which have little or no claim to occupy time or attention, we may divide the remainder into three classes:

1. Those which when strong enough to exert a sufficient germicidal action, are locally so irritating as to be harmful or unbearable. This class includes nitrate of silver, carbolic acid, chloride of zinc, iodine, chloral, potassium permanganate, salicylic acid, and creasote, all of which have been faithfully tried in many cases and by competent surgeons, the concurrent testimony being, that when used in sufficient strength to sterilize the discharges they produce an amount of local irritation and swelling, ardor urinæ, chordee, and even exceptionally urethral ulceration, that far outweighs any advantage to be derived from their antiseptic properties.

2. Those which are such feeble antiseptic agents that they cannot be depended upon to destroy all the bacteria found in urethral discharges. Among these may be mentioned resorcin, thallin, quinine, the sulphate and acetate of zinc, lanolin, sulphur waters, tannin, alum, hydronaphthol, and cadmium sulphate. The clinical and the experimental evidence coincide as to most of these drugs. Each has had its more or less enthusiastic advocates, but when given a wider trial has been found disappointing, while bacteriologists have shown that the germicidal action was either limited to a very few varieties of bacteria, or was slow and uncertain.

3. The third class includes a number of agents which, while open to the same objection of too feeble or too limited antiseptic action, have the additional drawback of insolubility in ordinary media and of occasionally becoming mechanically irritating from the formation of concretions. Among these are iodoform, calomel, bismuth subnitrate, oxide of zinc, and other insoluble powders.

It must not be supposed that this list is intended to be even approximately complete. It might be increased literally a hundredfold, and nothing could better demonstrate the blind absurdity of empirical methods than the dozens of ridiculous formulæ and the hundreds of useless drugs which have from time to time been recommended for use in the various forms of urethritis.

So far, however, as they are antiseptics they would be found in one or the other of the above classes.

There are certain agents which I have purposely omitted from this classification, but which, as ordinarily employed, might have been included with considerable propriety. These are corrosive sublimate, the sulpho-carbolate of zinc, boric acid, peroxide of hydrogen, and the salicylate of bismuth—the five drugs which in various combinations are, in my opinion, of the most practical value in attempting by topical treatment to secure asepsis in an inflamed urethra. Corrosive sublimate was used in the last century and in the early part of this century by Musitanus, Malow, Gardave, Benjamin Bell, John Hunter, Gutauer, and Swediaur. It then fell into disuse but was revived again in 1846 by Mueller von Berneck and employed spasmodically and unsystematically by a few surgeons until Neisser's discovery of the gonococcus gave the drug an extraordinary vogue. The records of its effects in different hands have thus far been most contradictory. While on the one side we have Leistikow, Lewin, Eichsbaum, Grandin, Chameron, Paul, Louissot, Keyser, and Macrae advocating its employment, on the other we have Diday, Wyeth, Dujardin-Beaumetz, Lewis, Finger, Auspitz, Bardezzi, Du Castel, Dreyfous, and Fournier who either recognize no advantage from its use or who record positive unfavorable experiences. Its use by the method of irrigation in solutions varying in strength from 1 in 60,000 to 1 in 10,000 has been popularized by the excellent work of Curtis, Halstead, Vanderpoel and Brewer, and their published results seem to warrant its further trial, especially in hospital and dispensary practice.

The method of deep irrigation or retrojection involving the use of a catheter has always been unsatisfactory in my hands in private practice. It involves too much expenditure of time on the part of both patient and surgeon, and in a genuine acute inflammatory urethritis is certainly objectionable on account of the irritation produced by even the softest instrument. Keyes and others have met with cases of cystitis, prostatic abscess, epididymitis and other complications which seemed to have been produced by this treatment used early in attacks of urethritis. Palmer has called attention to its inapplicability to private practice, and I can add my testimony to theirs, though I am aware that in forty-six cases treated by retrojection Dr. Brewer claims to have met with no such difficulty.

I found it much more satisfactory to order frequent injections from a large syringe with moderate force, using the strongest solutions that could be given without causing pain and preventing the fluid from passing too deeply by instructing the patient to sit upon a folded towel during injection so as to

occlude the membranous urethra. My results were, however, disappointing so long as I used this drug alone. In a certain proportion of cases, by no means a small one, any solution, however weak, seemed to irritate, giving rise not so much to pain at the time of injection as to subsequent increased ardor urinæ. In others it was well borne but had no influence upon the discharge; in others it aggravated it and those in which it caused very decided improvement were in the small minority. Rigid dietetic and hygienic directions were always given at the same time and were, as a rule, fairly well followed, as most of the patients were of good social position.

Boric acid recommended long ago by Hyndman, of Cincinnati, tried by Chameron, Du Castel and others with unfavorable results, was used by me in a considerable number of cases six or eight years ago and dropped on account of general negative results. It has been shown to have little effect on the gonococcus but it is quite unirritating and can be used in sufficiently strong solution to destroy many of the weaker forms of vegetable life. Its germicidal action is slow but continuous. I shall allude in a moment to its internal employment. I certainly improved my results when I added it to the bichloride injection in the proportion of fifteen to thirty grains to the ounce. I felt, however, the additional need of an astringent, and for a long time employed the sulphate of zinc, for which I have recently substituted the sulpho-carbolate (two to ten grains to the ounce of the mixture). By using a ten to fifteen per cent. solution of peroxide of hydrogen as a menstruum, I then found that I had a formula which, both clinically and experimentally, showed excellent antiseptic qualities. In exceptional cases of profuse catarrhal secretion the addition of bismuth salicylate was also of advantage.

The rigid application of the antiseptic principle by means of these drugs used in this way undoubtedly brought about a distinct gain in my results; complications were fewer and the time required for cure was shorter than when I was content with employing sedatives and astringents and endeavoring to meet symptomatic indications, but there was still much to be desired.

In quite a large proportion of cases it was not possible to use this injection, or any modification of it, for some days after the disease had reached its height. I was in the habit, under these circumstances of giving boric acid internally to sterilize the urine, combining with it potassium bromide and belladonna in a solution of potassium citrate.[1] This mixture never had any very marked effect upon the amount of discharge though it constantly moderated the ardor urinæ, chordee, and vesical tenesmus of the inflammatory stage. I was also in the habit, after those symptoms began to subside, of employing one or both of the two chief "anti-blennorrhagics," cubebs and copaiba, usually in a capsule in combination with tar and pepsin. There can be little doubt that the beneficial action of these drugs as well as that of their congeners, gurjun, sandalwood, kava kava, eucalyptus, and the various terebinthinates, is chiefly due to their antiseptic powers, which not only deprive the altered and partially sterilized urine containing them of many of its harmful properties, but cause it to exert a positively curative effect upon the suppurating mucous membrane.

This statement is borne out by a number of microscopic observations upon specimens of urine before and after the administration of these drugs and by culture and inoculation experiments which will form the subject of a later publication.

This represented my routine treatment for cases of recent urethritis up to the middle of last January. My average results were certainly better than they had ever been before I made antisepsis the guiding principle of treatment, though not equal to some of those said to have been obtained by the method of irrigation, which, as I have said, I have found impracticable in private practice. My attention was then attracted by an article in the *Gazette Hebdomadaire de Médecine et de Chirurgie* (January 4 and 11, 1890), by M. Ferdinand Dreyfous, on "Antisepsis of the Urinary Organs by the Internal Administration of Remedies."

His paper contained the results of his observations in seven cases of gonorrhœa in which salol had been administered in full doses both with and without admixture with cubebs and copaiba.

Sahli (*La Semaine Médicale*, April 7, 1886) had shown that the resultants of the intestinal decomposition of salol are salicylin and carbolic acid, which are eliminated by the kidneys. He had exposed to the air for some weeks the urine of a patient under the influence of salol without the least decomposition occurring. Nencki had made the same observations. Bouchard (*Thérapeutique des Maladies Infectieuses; Antisepsie*, 1889, p. 247) had called attention to the value of the simultaneous employment of several antiseptics in internal administration. M. Dreyfous on these grounds gave salol a trial, and arrived at conclusions which were rather sweeping, considering the very limited experience on which they were

[1] Palmer has shown (American Medical Practitioner, August, 1887) the value of boric acid in sterilizing the urine, having had but one case of urethral fever in forty urethrotomies, and that occurring in a patient in whom by accident the boric acid had not been administered. The well-known effect of quinine in preventing urethral fever is probably due largely to its antiseptic action and to its elimination through the urinary apparatus. (Reginald Harrison: Lettsomian Lectures, 1888.)

based, but which, taken in conjunction with the clinical histories of his patients, made so much impression upon me that I determined to investigate the subject. I accordingly asked Mr. Llewellyn, a prominent druggist, to prepare for me and keep on hand gelatin capsules, each containing

| | |
|---|---|
| Salol . . . . . | 3½ grains. |
| Oleoresin of cubebs . . | 5 " |
| Para balsam of copaiba . | 10 " |
| Pepsin . . . . . | 1 grain. |

I began at once to prescribe them in all forms and at all stages of urethral discharge, and, after a little time, in all cases in which much urethral or vesical instrumentation was required, giving from 6 to 8 daily. I have now used, as I learn from Mr. Llewellyn, nearly 10,000 capsules, and my case-books show that these have been prescribed in 97 cases. Throwing out of the latter the cases of urethrotomy, operations for vesical calculus, dilatation of strictures, etc. (in all of which I have come to regard it as of much value), there are left 72 cases in which the predominant symptom was urethral discharge. Of these, 19 were chronic and the results need not now be detailed, though I may say, *en passant*, that even among them in almost no instance did I fail to observe some benefit.

There remain 53 cases of recent urethritis, confined to the anterior urethra, but of all grades of severity. Of these, a distinct and unmistakable effect upon the discharge was produced in every instance but two, in which a violent indigestion prevented a thorough trial of the capsules. In 35, or about two-thirds of the whole number, the discharge almost or entirely disappeared during the first week after this treatment was begun. I have usually found, however, that it would return if treatment were withdrawn or even if the dose were greatly diminished; and although in 7 cases I depended from first to last upon the capsules and succeeded in effecting a cure in about seventeen days, in the majority of cases it was necessary to use the injection also. Since writing these notes I have seen an article by Mr. Ernest Lane (*The London Lancet*, March 22, 1890) giving his results in 50 cases of gonorrhœa, in which he used salol in doses varying from 5 to 30 grains, three times daily. An analysis of the 50 cases shows that 6 were cured, 25 showed considerable improvement, 15 were unaffected by the treatment, and in 4 the symptoms were aggravated. When he gave 20 grains three times daily and relied upon its internal administration without the assistance of injections he noticed an improvement in 20 out of 40 cases within a week, and 6 were cured completely in that time.

If I had seen Mr. Lane's paper earlier I would certainly have used larger doses of salol, and I have now ordered capsules to be made containing five and ten grains each. I am still of the opinion that the combination with cubebs and copaiba is of advantage, though I have tried salol alone in but a very few cases. I find it difficult to estimate with any accuracy or precision the lessening of the time required for a cure which has followed the systematic introduction into my practice of these antiseptic methods. I have already described the successive changes which I made in the topical remedy—the injection—each of which was an improvement, but I certainly noticed a much more marked and unmistakable advantage as a result of the employment of the capsules. In general terms I should say that these changes in my plan of treatment of acute urethritis have diminished the average duration of such cases by from one-third to one-half, and have even more decidedly reduced the discomfort and suffering, and lessened the frequency of complications.

In not one case in the whole number did gonorrhœal rheumatism (urethral synovitis) develop, and in three cases it was especially noteworthy that although they had never before been free from joint trouble during an attack of gonorrhœa, they had absolutely no threatening of it under this plan of treatment.

I have not attempted to give the average time of treatment in days for the reason that such statistics are worse than useless, they are misleading, unless they are based upon very large numbers of observations. A few days' difference in the time of beginning treatment, giving more or less opportunity for bacterial growth and for penetration into the deeper mucous layers, will necessarily make a still greater difference in the effectiveness of treatment and the rapidity of cure. So, too, cases of the kind upon which this paper is based, persons of fairly good position and intelligence, should not be classed with the ordinary applicant for dispensary or hospital treatment. I have, therefore, confined myself to this somewhat sketchy statement of my experience, which is, as yet, not large enough to warrant more definite assertions. It, however, taken with the general testimony upon the subject, justifies, in my opinion, the following conclusions, some of which are closely parallel with those M. Dreyfous had reached after even less extended observation:

1. The microscopical and experimental evidence in favor of the bacterial origin of gonorrhœa is confirmed by clinical and therapeutical experience.

2. The results of antiseptic treatment have not been so uniformly successful as would be expected, on account of—*a*, the anatomical and physiological peculiarities of the male urethra; *b*, the difficulty of applying sufficiently energetic local antisepsis; *c*,

the necessarily intermittent character of such applications, and *d*, the failure to combine with the topical treatment appropriate internal medication.

3. No one antiseptic agent can be depended upon, in the strength at which it can be borne by the inflamed urethral mucous membrane, completely to sterilize the discharges and the suppurating surfaces. A judicious combination of several antiseptics, if not essential to success, is at least of considerable value.

4. The internal administration of salol in conjunction with copaiba and cubebs renders the urine aseptic and probably antiseptic, so that it acts as an exceptionally thorough and efficient antiseptic injection, shortening the duration of the disease and diminishing the frequency and severity of the complications.

5. The production of urinary asepsis by internal medication is of considerable value not only in the inflammatory affections of the genito-urinary tract, but in the preparation of the patient for operation upon those organs and in the after-treatment.

## THE EXAMINATION OF DRINKING-WATER WITH SPECIAL REFERENCE TO ITS RELATION TO TYPHOID FEVER.

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*Introduction.*—When the Michigan State Board of Health asked the Regents of the University of Michigan to build and equip a laboratory of hygiene, the objects of the laboratory were stated to be as follows:

1st. Original investigation into the causation of disease.

2d. The examination of samples of food and drink, at a nominal cost, on the request of local health officers; and,

3d. The instruction of students in hygienic investigations.

In accordance with the several objects, as stated above, health officers have frequently called upon the Director of the Laboratory to decide as to the potability of samples of drinking-water. Some analyses have been made for those residing in other States.

*Method of Examination.*—Naturally I have been anxious to make the reports on these waters as thorough as possible. I regard the following conditions as essential to a satisfactory examination of drinking-water:

1. The report must be returned to the health officer within a week, at most, after the water has been received. Typhoid fever appears in a village and a certain water supply is suspected. The health officer sends a sample of the water, and he will hardly be satisfied if he has to wait a month or longer for the report. At first I isolated the germs by means of plate cultures and inoculated animals with pure cultures of each germ. This method was pursued with the first thirty-eight samples given in the table. But the time required was too long, and the following procedure has been adopted: In all cases where the samples are collected by the laboratory assistants, a sterilized flask closed with cotton is used. In other instances, when I am informed by a health officer that he desires the analysis of a water, I send him a sterilized bottle, or instruct him how to sterilize a bottle. In other cases, the water comes usually in a new bottle or jug, which has been well rinsed with the same water. As soon as the water is received, plate cultures are made and a test-tube of beef-tea is inoculated with one drop of the water. This tube is placed in an incubator and kept at 37° C. for twenty-four hours. Then twenty drops of the beef-tea culture are injected with a sterilized syringe into the abdominal cavity of a white rat or guinea-pig. In some instances rabbits have been used, and with these the amount injected has varied from twenty to sixty drops, according to the size of the animal. If the water contains a pathogenic[1] germ the animal dies, usually within twelve hours. A post-mortem is made, the gross appearances noted and plates are prepared from the spleen, liver, kidney, and sometimes from the blood. After twenty-four hours longer these plates have generally developed (in some instances a longer time is required), and they are compared with the plates made directly from the water. On the plates made directly from the water the germs are counted and their general appearance noted after twenty-four, forty-eight, and seventy-two hours. In the meantime the chemical analysis is completed, and under favorable circumstances the report can be made three days after the water has been received. A week furnishes all the time required in any case. The germs taken from the plates can then be studied at leisure.

2. The second condition, which I regard as essential, is that the report, especially when the water is condemned, should be of such a nature that it will convince the average mind. No method of chemical analysis, with which I am acquainted, will do this. The ordinary bacteriological examination is no better. If the analyst reports a thousand germs in a drop of water, and stops at this, he will find that his report is not satisfactory. Immediately he will be asked if these germs are capable of doing any harm. It is true that the fact that a germ kills a rat does not furnish positive proof that it will cause

[1] The word *pathogenic* is used in the sense of *capable of producing death*, and it does not mean the production of any specific disease.

disease in man, but it is the most convincing proof that I know of that the water is not safe to drink. The search for certain pathogenic germs has not been attended with great success. The detection of Eberth's germ, by those who believe in its existence in all drinking-waters which cause typhoid fever, is admitted to be very difficult. I have only to refer to the recent work of Tiemann and Gärtner on the examination of drinking-water in proof of this assertion.

SAMPLES OF DRINKING WATER EXAMINED IN THE HYGIENIC LABORATARY OF MICHIGAN UNIVERSITY, FROM OCTOBER 1, 1888, TO MAY 1, 1890.

| No. | Water sent from | Kind. | Class. | Free ammonia. | Albuminoid ammonia. | Chlorine. | No. of germs. | Kind of germs. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| 1 | Omaha, Nebraska | Well | Second | 0.168 | 0.118 | 129 0 | 50 | No. 10 was found in all the Omaha samples; No. 8 in sample No. 4; No. 25 in Nos. 1, 3, and 4; Nos. 9 and 30 in samples 5, 7, 8, 9, and 10; undescribed germs were found in samples 2 and 4. | Pure cultures of each of these germs in beef-tea, twenty-four hours old, were injected into the abdominal cavity of rabbits and rats; all were without effect. |
| 2 | " " | " | " | 0.050 | 0.124 | 97.0 | 46 | | |
| 3 | " " | " | " | 0.664 | 0.230 | 12.0 | 120 | | |
| 4 | " " | " | " | 0.162 | 0.116 | 45.0 | 50 | | |
| 5 | " " | " | " | 0.094 | 0.112 | 23.0 | 26 | | |
| 6 | " " | " | " | 0 028 | 0.090 | 34.0 | 18 | | |
| 7 | " " | " | " | 0 022 | 0.102 | 48.0 | 38 | | |
| 8 | " " | " | " | 0.172 | 0.088 | 48.0 | 36 | | |
| 9 | " " | " | " | 0.054 | 0.068 | 98.0 | 20 | | |
| 10 | " " | " | " | 0.032 | 0.096 | 8.0 | 8 | | |
| 11 | Coldwater, Michigan | " | " | 0.270 | 0.043 | 8.0 | 500 | | |
| 12 | " " | " | " | 0.015 | 0 025 | 14.0 | 712 | Nos. 6, 8, and 10 were found in these waters | Inoculations of rabbits were without effect. |
| 13 | " " | " | " | 0.270 | 0.040 | 15.0 | 370 | | |
| 14 | " " | " | " | 0.025 | 0.040 | 12.0 | 580 | | |
| 15 | Ida, " | " | " | 0.910 | 0.120 | 155.0 | 2000 | No. 10 | Inoculation of rabbit was without effect. |
| 16 | Brown City, " | " | " | 0.220 | 0 096 | 8.0 | 400 | Nos. 8 and 10 | Inoculations of rabbits were without effect. |
| 17 | Kalamazoo, " | " | Third | 0 c41 | 0.084 | 7.0 | 83 | " " | " " " " " |
| 18 | " " | " | " | 0 020 | c.066 | 7 0 | 60 | " " | |
| 19 | Mt. Pleasant, " | River | Second | 0.094 | 0.208 | 79.0 | 3000 | | Inoculations of rats were without effect. |
| 20 | Gambier, Ohio | Well | " | 0.065 | c.062 | 240.0 | 683 | Nos. 6, 8, 9, 10, 25, and 30 | Inoculations of all the germs on rats and rabbits were without effect. |
| 21 | Allegan, Michigan | " | " | 0.206 | 0.148 | 33.0 | 300 | No. 10 | Inoculation of rabbit was without effect. |
| 22 | Three Rivers, " | " | Third | 0.070 | 0.046 | 9.0 | 25 | | |
| 23 | " " " | " | " | 0.108 | 0.072 | 7.0 | 90 | " " | Inoculations of rats and rabbits were without effect. |
| 24 | " " " | " | " | 0.024 | 0.054 | 7.0 | 22 | | |
| 25 | Allegan, " | Hydrant | " | 0.220 | 0.066 | 31.0 | 40 | " " | Inoculations of rats were without effect. |
| 26 | " " | Artesian | " | 0.272 | 0.056 | 32.0 | 25 | | |
| 27 | Coldwater, " | Well | " | 0.024 | 0.038 | 14.0 | 16 | " " | Inoculation of rat was without effect. |
| 28 | Kalamazoo, " | " | " | 0.144 | 0.058 | 9 0 | 280 | Nos. 8 and 10 | Inoculations of rabbits were without effect. |
| 29 | Tecumseh, " | " | Second | 0.037 | 0.078 | 10.0 | 40 | No. 10 | Inoculation of rat was without effect. |
| 30 | Ishpheming, " | Lake | Third | 0.792 | 0 418 | 6 0 | 455 | | |
| 31 | " " | " | " | 0.036 | 0.222 | 2.0 | 50 | Nos. 8 and 10 | Inoculations of rats and rabbits were without effect. |
| 32 | " " | " | " | 0.038 | 0.208 | 3.0 | 48 | | |
| 33 | Three Rivers, " | Well | " | 0.900 | 0.036 | 0.9 | 30 | | |
| 34 | " " " | " | " | 0.060 | 0.102 | 0.7 | 25 | No. 10 | Inoculations of rats were without effect. |
| 35 | " " " | " | " | 0.016 | 0.062 | 1 0 | 22 | | |
| 36 | St. Louis, " | Reservoir | " | 0.060 | c.210 | 23.0 | 8400 | Nos. 6, 7, 8, 9, and 10 | Inoculations of rats and rabbits were without effect. |
| 37 | " " | Pine River | " | 0.044 | 0.320 | 24.0 | 1008 | Nos. 6, 8, and 10 | |
| 38 | Climax, " | Well | Second | 0 006 | 0.084 | 4.5 | 105 | Nos. 8 and 10 | Inoculations of rats were without effect. |
| 39 | Grand Rapids, " | River | Third | c.030 | 0.134 | 10.0 | 36000 | Bacillus D. | Rabbits inoculated with No 39 died, while those inoculated with No. 40 were not affected. |
| 40 | " " " | " | " | 0.024 | 0.075 | 6.0 | 120 | | |
| 41 | Ann Arbor, " | Cistern | First | 2.223 | 0.908 | 3.0 | 3000 | Bacillus A. | Rats, guinea-pigs, and rabbits inoculated with this germ died. |
| 42 | Negaunee, " | Lake Teal | " | 0.228 | 0 368 | 30.0 | 920 | In No. 46, germs Nos. 8, 10, 25 were found; in Nos. 42, 43, 44, 45 was found, in addition to the above, bacillus E | Rabbits inoculated with bacillus E died. Those inoculated with the other germs from the same waters were not affected. |
| 43 | " " | " " | " | 0.232 | 0.460 | 30.0 | 1240 | | |
| 44 | " " | " " | " | 0 296 | 0.448 | 40.0 | 772 | | |
| 45 | " " | Mine | " | 0.312 | 0.236 | 140 0 | 2060 | | |
| 46 | " " | Sand shaft | Third | 0.042 | 0.120 | 40.0 | 772 | | |
| 47 | Mendon, " | Well | Second | 0.019 | 0.048 | 4.0 | 25 | No. 10 | |
| 48 | " " | " | " | 0.020 | 0.210 | 34.0 | 100 | Nos. 8, 10, and 25 | Rats were not affected. |
| 49 | Deerfield, " | " | First | 0.508 | 0.158 | 695.0 | 898 | Bacillus C. | Rats inoculated with this germ died. |
| 50 | Northville, " | " | Second | 1.136 | 0.732 | 6.7 | 1550 | No. 10 | Inoculation of rats was without effect. |
| 51 | Iron Mount'n, " | " | First | 1.256 | 1.920 | 44.0 | 1500 | | |
| 52 | " " " | " | " | 0.384 | 0.840 | 20.0 | 1350 | | |
| 53 | " " " | " | " | 0.784 | 0.960 | 52 0 | 1200 | No. 10 and bacillus B were found in all these waters. | Rats, guinea-pigs, and rabbits inoculated with bacillus B died. |
| 54 | " " " | " | " | 0.904 | 1.360 | 16.0 | 1700 | | |
| 55 | " " " | " | " | 0.456 | 0.944 | 12.0 | 850 | | |
| 56 | " " " | " | " | 0.464 | 1.256 | 28.0 | 950 | | |
| 57 | Ann Arbor, " | Cistern | " | 0.318 | 0.262 | 6 0 | 1200 | Bacillus A. | Rats, guinea-pigs, and rabbits inoculated with bacillus A died. |
| 58 | " " " | " | " | 0 492 | 0.504 | 9.0 | 1770 | | |
| 59 | Rankin, Illinois | Well | " | 3.512 | 0 332 | 24.0 | 738 | No. 10 and bacillus B | Bacillus B is fatal to rats, guinea-pigs, and rabbits. |
| 60 | Pomfret, Conn. | " | Second | 0 104 | 0.116 | 4.0 | 151 | Nos. 8 and 10 | Inoculations of rats were without effect. |
| 61 | Sherwood, Mich. | " | First | 0.194 | 1.160 | 7.0 | 4116 | No. 34 | Rats inoculated with this germ were not affected |
| 62 | Soudan, Minnesota | Lake | Third | 0.144 | 0.488 | 3.0 | 428 | No. 30 and bacillus E | Rats inoculated with E died. |
| 63 | " " | " | " | 0.132 | 0.488 | 4.0 | 459 | " " | Rats inoculated with No. 30 were not affected. |
| 64 | " " | Well | Second | 0.120 | 0.090 | 2.0 | 28 | " " | |
| 65 | " " | " | " | 0.072 | 0.124 | 2 0 | 4 | Bacillus A | Rats inoculated with A died. |
| 66 | Lake City, Mich. | Lake | Third | 0 154 | 0.280 | 3.0 | 124 | No. 30 | Rats were inoculated without effect. |
| 67 | Iron Mount'n, " | " | " | 0.260 | 0 270 | 4.5 | 9 | " " | " " " " |
| 68 | Ironwood, " | Well | " | 0.176 | 0.248 | 9.9 | 12 | " " | " " " " |
| 69 | Ann Arbor, " | Spring | " | 0.012 | 0.016 | 2.5 | 0 | | |
| 70 | " " " | " | " | 0.044 | 0.120 | 4.0 | 4 | No. 30 | Rats were inoculated without effect. |
| 71 | " " " | " | " | 0 050 | 0.080 | 6.0 | 0 | | |
| 72 | " " " | " | " | 0.090 | 0.214 | 4.0 | 6 | No. 30 | |
| 73 | Leslie, " | Well | Second | 0.536 | 0.840 | 14.0 | 104 | Nos. 10 and 30 | " " " " |
| 74 | " " | " | " | 1.144 | 0.984 | 12.5 | 2940 | | |
| 75 | Ann Arbor, " | " | Third | 0.064 | 0.064 | 17.0 | 150 | No. 34 | " " " " |
| 76 | " " " | " | " | 0.068 | 0.070 | 6.0 | 60 | " " | " " " " |
| 77 | " " " | Spring | " | 0.124 | 0.194 | 3.5 | 295 | No. 30 | " " " " |

There is no need of my going into detail concerning the method of chemical analysis. I have nothing new to offer in that line, and have practically followed the methods of Wanklyn. The chemical analysis is much more extensive than is shown in the table. I have given only the results of the estimation of free and albuminoid ammonia and chlorine, because much importance has generally been attached to these by chemists. In addition to the figures given in the table, a record is made of the physical properties, including hardness, the amount of residue inorganic and organic, the amount of oxygen consumed in the permanganate process, of sulphates, earthy bases, nitrates and nitrites.

*Explanation of the Table.*—The waters are divided into three classes. The first class contains those which are supposed to be causing typhoid fever at the time the water is sent, or only a few days before the water is sent. In other words, the water has been sent on account of the present existence of typhoid fever among those using it. In the second class I have placed those waters which are not supposed to be causing typhoid fever at the time, but which are suspicious on account of unsanitary surroundings, or on account of former sickness among those using it. In the third class are given those waters which have not been suspected of causing disease, and the sanitary surroundings of which are not said to be especially bad. Several of this class have been sent for analysis, in view of their possible selection for the future water-supply of cities and villages. The figures given in the chemical part of the table represent parts per million, the chlorine being calculated as sodium chloride. The number of germs in one drop of the water is shown. In the column "kind of germ," the figures refer to the corresponding ones given in the tables of Eisenberg (*Bakteriologische Diagnostik*, Zweite Auflage), and the letters refer to germs which have proved fatal to the animals inoculated. In all cases, even in those in which the germ was recognized as one well known to be common in water, animals were inoculated. I am well aware of the fact that *old* cultures of some of the ordinary water germs may, when used in large amount, kill animals, and this has been avoided by using cultures only twenty-four hours old. Moreover, a number of the ordinary germs of water will, in quantities of from ten to twenty drops of cultures only twenty-four hours old, kill mice, therefore this animal has not been used. The rat seems especially suitable for this work, on account of its resistance to the ordinary water-germs. I think that this animal is more suitable than the rabbit or guinea-pig.

Other waters have been partially examined, some chemically only, and others bacteriologically only, but I have given only those which we have examined both chemically and bacteriologically, and only those the germs of which have been tested by inoculations.

*Remarks on the Table.*—It will be seen that the waters are distributed in classes as follows: In the first, 16; in the second, 29; in the third, 32. In 15 out of the 16 typhoid waters, germs pathogenic to the lower animals were found. The exception was water No. 61, from Sherwood, Mich. The number of germs in this water, as is seen, was large. The germs were all of one kind, No. 34 of Eisenberg, the white non-liquefying bacillus of water. Beumer and Peiper have shown that cultures of this germ kill mice, but that the effect is due to the poisonous chemical products of the germ in the cultures, and not due to the growth and reproduction of the organisms within the animal. Rats were repeatedly inoculated with twenty-four hours' cultures, and notwithstanding the fact that the amount employed was doubled, no effect was produced. Dr. Nelthorpe, who sent the sample of water, wrote as follows:

"The well is 40 feet deep, and distant 40 feet from a privy, and 28 feet from a hog-yard. The well was an old open one, but a gas pipe was put down 10 feet below the bottom of the well, the open part not being filled. The whole family of six children, and father and mother, have been attacked. So far, only one death has occurred."

All the circumstances being taken into consideration, the water was condemned. It will be observed that chemically it was very bad.

Out of the 29 samples of the second class, only one, No. 65, from Soudan, Minn., contained a pathogenic germ. C. T. Waters, who sent the sample, wrote:

"The well is 80 feet deep. It is situated on a considerable knoll, with still higher ground on one side, on which is a privy vault about 200 feet distant. Slightly lower than the surface of the well on the other side, at a distance of about 40 yards, is another privy."

The number of germs deserves attention. Chemically, the water was among the best. Its use was not advised, notwithstanding the chemical showing and the small number of germs. The microörganism will be described later. If I was right in condemning this water, the inadequacy of a chemical analysis and the ordinary bacteriological examination is here well demonstrated.

Of the 32 samples of the third class, two contained germs pathogenic to the lower animals, and these may be worthy of comment. No. 39 was from Grand River, and no one acquainted with the course of that river will claim that the sanitary surroundings are free from suspicion, a number of small cities discharging sewerage into it. I have hesitated about the class in which this water should be placed, and finally I classified it as I have, simply because the water was not sent to me on account of the unsani-

tary surroundings, but for the purpose of comparing it with No. 40, and in this comparison lies an interesting fact. No. 40 is said to be obtained from springs, but, as I understand it, the so-called springs are only wells sunk near the bank of the river, and into which the river-water filters. The two waters differ only in degree. They contain the same germ, which probably does not kill by growth in the body of the animal, but by the formation of poisonous chemical substances. In No. 39 the number of germs was 36,000, in No. 40 only 120. It is probable that even within twenty-four hours the larger number of germs produced enough of these poisons to kill the animal.

The next member of this class which contains a pathogenic germ is No. 62. This water was from Lake Vermilion, Minn., and this sample was taken from near the shore—water No. 63 comes from the middle of the same lake. It will be seen that in their chemical composition and number of germs, these samples differ but little.

Each of the waters of the first class deserves brief description. The first of these is No. 41. Of the cases of typhoid fever supposed to be due to this water I had personal knowledge. First, a girl of sixteen years was attacked, and I was called in attendance. I inquired concerning the drinking-water, and was told that the hydrant water *only* was used. I could not believe that this water was the cause of the fever, but ordered that all water used in house should be boiled. I was asked if the disease was contagious, and replied in the negative, but having in consideration the comfort of my patient, advised that two young brothers and a girl visiting at the house should be kept away from the sick-room. Within eight or ten days one of the brothers came down with the fever, and we learned that the visitor, who had gone to Cincinnati, was suffering from the same disease. Of course, the mother now claimed that the disease was contagious. Believing that there must be some local cause, I examined the house throughout, but without satisfaction. A little later, a young man rooming in the house was attacked. By this time the first patient who had been continuously delirious for three weeks, had become rational. When asked about having used any other than the hydrant water, she stated that she always used the cistern water to clean her teeth, and had also used it in a nasal douche, and in spraying her throat. The brother and the student also remembered that they had occasionally drank the cistern water.

The examination of the water was immediately undertaken, and, as is seen from the table, it was found to be exceedingly bad chemically, and in number and kind of germs. The stools of two of the patients were then examined and the same germ found so abundantly in the water was recognized in the stools. Of course, I cannot say that this germ was the cause of the disease. Fortunately, all of these cases finally recovered and there was no opportunity to examine their spleens. I cannot refrain from mentioning a few facts concerning the symptoms. Delirium was a marked and persistent feature; it was not the low muttering delirium, but wild, causing the patients to attempt to get out of bed. The fever ran forty-two days. There was no eruption over the abdomen. The patients who drank of the waters, Nos. 57 and 58, were also under my care, and, in general, the symptoms were similar to those given, but not so grave.

In regard to the outbreak of typhoid fever at Negaunee (waters 42 to 45), Dr. Lombard has furnished me with the following statements: The first case appeared in July, 1889, and the last in January, 1890. The total number of cases reported was 358, of which 32 proved fatal.

Negaunee takes its water-supply from Teal Lake, which covers about 600 acres of land and is supposed to be fed by underground springs. In 1886, Dr. Lombard asked the State Board of Health to assist him in preventing the contamination of the water of this lake. Drs. Avery and Baker visited Negaunee for this purpose, and from their report the following quotation is taken:

"Midway between the natural outlet and the water-works and on the border of the lake is situated a slaughter-house, where the principal part of the slaughtering for the city is done. A few rods nearer the water-works is a smaller slaughtering-house. Both of these places drain directly into the lake at certain seasons, and through a small quantity of sand at other times. The houses and their surroundings were as filthy as they could well be made. The stench in and around them was simply unbearable. In the larger house there was a great pile of heads, the accumulation of two or three years' killing and in various stages of decomposition. In another corner was a pack of salted hides. On and under the floor there was decomposed and decomposing blood; and outside the building wagon loads of refuse, rotting and stinking."

At the solicitation of these gentlemen, the slaughter-houses were removed. How thoroughly the shores of the lake were cleaned we are not informed, but of one thing we can be quite certain, the large amount of organic matter which was poured into the lake for years was not removed, save as in solution, or as finely suspended matter, it passed through the natural outlet or was pumped into the water-supply, and on this point a further short quotation will be of interest:

"At the time of the visit of your Committee, not more water than would pass through a two-inch pipe, with an incline of one inch to the rod, was running from the outlet, while at the works there was being pumped about 600,000 gallons per day, practically making the outlet of the lake at the water-works."

In regard to the Deerfield water (No. 49), Dr. Yale wrote me the following facts:

"There were seven cases of typhoid fever among those using the water of the well. The water comes up from a four-inch bore 80 or 90 feet into a reservoir 18 feet deep. As the reservoir is not well covered, it is probable that something has fallen into it."

The large amount of salt in this water is of no significance on account of the nearness of the saline deposits to the surface at that place.

The Iron Mountain waters (Nos. 51 to 56) were from shallow wells, and as I have elsewhere[1] gone into detail concerning the condition of the drinking-water of this place, I will not say more on the subject.

The Rankin water (No. 59) was from a drive well 218 feet deep, but the bore is larger than the pipe and surface water could easily pass into the well in this way. There is a barnyard near.

*Remarks on the pathogenic microörganisms* — *Bacillus* A.—This germ was found in waters 41, 57, 58, and 65. It is a short motile bacillus which does not liquefy gelatin, and its growth on potato presents a white, raised, moist surface. It produces no gas, takes the ordinary stains fairly, and gives a positive result with Gram's method. Of 6 full-grown rabbits each inoculated in the abdominal cavity with sixty drops of a twenty-four hour culture, all died. Of 2 rabbits, each of which were given subcutaneously twenty drops of this culture, neither was affected. Of 33 rats which had twenty drops of this culture, all died within twenty-four hours. Of 2 rats which were inoculated subcutaneously, both died after ten days. Of 8 guinea-pigs inoculated in the abdomen, all died; and a like result followed the subcutaneous inoculation of guinea-pigs. Cultures of this germ were tested along with similar cultures of bacterium coli commune, and the former appeared to be slightly more pathogenic with rats and guinea-pigs and less so with rabbits than the latter.

In every case of death after inoculation with this germ, plate-cultures were made from the spleen, liver, and kidney, and in many instances from the blood of the heart, and without exception the plates showed abundant growths. Therefore, this germ not only produces death, but is capable of living in the rat for ten days.

The post-mortem appearances vary with the time which elapses between the inoculation and death. In cases dying within twenty-four hours the intestines are, as a rule, slightly reddened, the liver, spleen, and kidneys engorged, and the heart has invariably been found in diastole. In cases which survive for several days there may be ulceration of the intestine, the spleen and kidney are enlarged, and the liver dark. In a few instances, all of which, strangely enough, have followed subcutaneous inoculation on the back, a marked peritonitis has been found after death. The same fact has been observed with bacillus B. In no instance has suppuration resulted from an inoculation.

*Bacillus* B.—This is also a short motile rod, which in cultures from four to six weeks old shows some liquefaction of the gelatin. It is certainly a slowly liquefying germ. It sometimes forms gas along the line of the stick. This occurs, if at all, during the first few days of growth and long before liquefaction begins. It forms a white, raised growth on potato. The ordinary stains are taken readily, and Gram's method yields positive results.

This germ agrees very closely with bacillus A in its pathogenic properties and in the post-mortem appearances induced—so closely, indeed, that it will not be necessary to repeat details. Yet the two germs are wholly distinct morphologically, and, as we shall see later, the symptoms produced by their chemical poisons differ. Moreover, this germ has, in the few instances in which it has been tried, failed to kill rats when inoculated subcutaneously, though it is fatal to guinea-pigs when employed in this manner. The plates prepared from the organs of animals killed with this germ have invariably yielded numerous colonies. The germ is also found in the blood.

Rats were inoculated in the same manner in every particular with bacilli A, B, and Eberth's germ. A and B killed a larger per cent. of the animals than Eberth's germ, but the post-mortem appearances were the same in the three sets of cases.

*Bacillus* C rapidly liquefies gelatin, the liquefaction beginning at the surface. It forms gas, is stained by the ordinary agents and by Gram's method. It resembles Eberth's germ in the formation of an invisible growth on potato, but in no other respect.

Some 20 rats were killed with this germ. The minimum fatal amount of the twenty-four hours' culture for white rats seemed to be between 10 and 15 drops. One strange observation was made. Cultures nine or ten days old are apparently less virulent than those only twenty-four hours old. Moreover, the germ decreases in virulence as it is grown in the artificial media, until in about the sixth generation it may be employed in four times the quantity first used without any effect.

The post-mortem appearances are quite different from those observed with A and B. The intestines are reddened but the liver and spleen are tarry black. Cultures from these organs show numerous liquefying colonies.

*Bacillus* D, as has already been noted, is probably not pathogenic, but kills on account of the chemical

[1] THE MEDICAL NEWS, vol. lii. p. 92 et seq.

poisons rapidly developed. Further study of this germ is needed before statements can be made concerning it. I can only say that it does not correspond with any germ given in the tables of Eisenberg.

*Bacillus* E is non-liquefying. It grows slowly in stick cultures, and forms a scarcely visible growth on potato. It decreases in virulence with artificial cultivation, and while none of the other germs in these waters showed any pathogenic properties in pure cultures, beef-tea inoculated with a drop of the water never failed to kill rabbits and rats, while bacillus E in pure cultures frequently failed to produce death. Moreover, after the death of these animals inoculated with the mixed culture, bacillus E was the only germ found living in the organs. The following quotation from my notes will illustrate this: October 28, 1889, a large rabbit was inoculated with sixty drops of beef-tea, to which one drop of the Negaunee public water had been added. October 29th. Animal found dead at 9 A. M. Post-mortem showed small intestines and mesentery highly inflamed; no other changes observable. Plates made from spleen, liver, and kidney showed pure culture of germ marked "Negaunee."

It is possible that this is Eberth's germ, though of this I am not certain. Unless this be the exception, I have not found Eberth's germ in any of the waters.

*The chemical poisons produced by bacilli* A *and* B. —Cultures of these germs, after being heated to 100° C. for one hour, lose all their poisonous properties. I concluded from this that their chemical poisons are destroyed at this temperature, and determined upon the following method of isolating them: eight quart flasks of beef-broth were inoculated with each germ and kept in the incubator, at 37° C., for ten days. They were then filtered twice through heavy Swedish filter-paper, and during the last filtration the filtrate was allowed to fall into a large volume of absolute alcohol. A copious flocculent precipitate was produced; the supernatant fluid was decanted and the precipitate repeatedly dissolved in water and reprecipitated with absolute alcohol. Finally, the precipitate was collected in a dish and immediately placed in a desiccator over caustic potash (or over sulphuric acid, though the former seems to prevent decomposition better). After drying, small quantities (about the size of a large pea) were dissolved in water and injected into rats, producing death in about three hours. These poisons are albumins. They are soluble in water, from which they are not precipitated either by heat or nitric acid, singly or combined. They are precipitated by saturating the aqueous solution with ammonium sulphate, and are, therefore, not peptones. They are not precipitated by carbonic acid gas, or by sodium sulphate to saturation, and are, therefore, not globulins. The aqueous solutions respond to the xantho-protein and biuret reactions.

Chemically, I can so far distinguish no difference between the poisonous albumins produced by the two germs, but physiologically there is a great difference. That from bacillus A produces in a few minutes complete paralysis of the voluntary muscles, and the animal lies perfectly limp, with rapid breathing, until death occurs. The proteid of bacillus B is equally poisonous, but it induces no paralysis of the voluntary muscles, and death apparently results from gradual heart failure. After death from these poisons no abnormal appearances could be detected. The heart was found in diastole. Plates were made from the kidney, spleen, and liver, but remained sterile.

It is absolutely necessary in obtaining these poisons that they should be quickly transferred to the desiccator. Only a few minutes' exposure to the air while moist converts them into hard, brown, leathery substances soluble in water only with great difficulty and wholly inert. The further study of these poisons promises to be of interest.

Whether or not any crystalline poisons are formed by these germs I have not yet determined.

I have intentionally refrained from offering any theories, but have simply stated the facts gathered in the study of these waters. There are from 350,000 to 400,000 cases of typhoid fever in the United States yearly. Medical men believe that these are almost wholly due to impure drinking-water. If this be true, it does seem that with all the advances which have been made in methods of chemical and bacteriological study we should be able to detect the harmful agent or agents in these waters.

I conclusion, I desire to thank my able assistant, Dr. F. W. Brewer, and my students, Mr. Churchill, Miss Schryver, and Miss Hinkley, for their valuable assistance, without which the work could not have been done.

## A RETROSPECT OF ABDOMINAL SURGERY.[1]

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UNDER this title I wish briefly to call attention to the accomplishments of abdominal and pelvic surgery, together with the influences that have led to such successful results in the handling of diseases of the peritoneum. Among the first of these influences must be considered early operations in the hands of those best able to appreciate the necessity, if the ultimate safety of the patient is to be considered. In the light of proven statements of operative surgeons, physicians in general practice have no longer the right even to question the expe-

[1] Read before the American Medical Association, May 20, 1890.

diency and necessity of referring women afflicted with abdominal growths, small or large, at once to the attention of the skilled surgeon. They have no right to *suppose* in the case at all. They are not and cannot be judges of the situation, and by presuming or pretending to be so they reflect ultimately on their skill as physicians and on their unselfish honor as men. It must be taken as a corollary to the now established propositions of abdominal surgery, that in the large majority of cases the earlier the operation the greater the chance for success. That there may be an occasional exception to the rule is no excuse for delay, for if now and then an exception occurs, it is such as one not skilled in abdominal diagnosis is able to decide upon.

Looking back over the field of abdominal and pelvic surgery, nothing can strike the practical surgeon with greater force than the change that has come over opinion as to what is and what is not a justifiable operation, a question which is of paramount importance in determining the choice of cases. To know what to operate upon and what to refuse as inexpedient, impossible, or useless, is primarily of the greatest importance to the patient, to the physician, and to the surgeon. Up to a period measured scarcely by years it was the custom to bring so-called pelvic "abscesses" to the surface, stitch them there, and leave them to drain away. Extra-uterine pregnancies were not operated upon at all, or most exceptionally. When they were interfered with, it was only in a tentative way, that had nothing exact in its methods, and nothing tangible in its results except the death of the patient. There was preliminary puncture, with subsequent hæmorrhage or peritonitis, or there was injection of morphine into the sac, or a trial of electricity. The last two procedures are still flapping their ghastly wings like vultures over their graveyard records and inviting new victims. Although so prominent a man as Barnes still refers to the possibilities of morphine injections into the extra-uterine sac, by practical surgeons the suggestion must be thrown aside. As to electricity, if it has a place at all it is where it is not needed, and where surgical interference is by all odds the safest and surest method, both of doing no harm and of leaving no post-operative complications. Practical surgery like all other sciences, must leave pure theory behind. A theory may be enticing and brilliant, yet if it is not substantiated by facts and if it does not prove its worth it is left behind. Pure theory looks at facts from the outside, not from experience The theorist has his mind full of ancient pathology, constantly contrasts it with the more modern exposition of the same subjects, and is lost in a maze of bewilderment that the two should be so widely at variance. He may be on the verge of an exact diagnosis, or at the point of urging radical surgery to relieve the patient, when the wraith of defunct pelvic cellulitis rises hazily before him, obscures his mental vision, and drives him back to opium, hot water, and poultices. It is still no unusual occurrence to have inflammatory tubal troubles dubbed "cellulitis," and to have some guiding star sought to point out the difference between a condition that is benign because it does not speedily kill, and the same condition further advanced, yielding only to the knife. It should be strongly impressed upon the general surgeon and the general practitioner, that operative gynecologists do not find pelvic cellulitis. Electricians, experimentalists, and conservative procrastinators have a monopoly of this condition. It is not to be forgotten in connection with this that diagnosis with the latter men is the most exact part of their work. Their refinements are such as are not attained by any other class of operators, and where they can easily establish the existence of microscopic lesions of course they can cure infinitesimal disease, which has its chief exponent in pain. Pain apart from lesion should rarely in gynecology be assumed as a proper cause for operation. And among careful surgeons, the reproach of indiscriminate operation so soon as a woman comes complaining of pain, is unmerited. It is to be remembered here that we deny the right of the word conservative to be misapplied to a set of surgical pretenders, who are not conserving, but only riding on a wave of popular favor, because the word conservative is construed into meaning "careful," and that the conservative surgeon is the safer man to deal with is assumed. Let us see. To a man these "conservative" men are without experience. If they have attempted radical surgery they have failed, and abandoned it on that account. If they are young men they have been dragged into the electrical vortex, and while they would laugh at witches and witchery, they ascribe greater accomplishments and mysteries to the electrical current than was ever done to Aladdin and his wonderful lamp. Electropath, hydropath, homœopath—of the three the greatest humbug is the electropath. What conclusions are we to reach then concerning the wonderful cures attributed to this and that panacea?

Simply this, that the cure rests upon an incorrect or fallacious diagnosis, or that a temporary improvement of a condition which often occurs even without treatment has been misinterpreted a cure, or that after a short term of treatment a cure is presumed because the patient disappears, or that an incorrect report has been given, which unfortunate as it is, is too often the case. Now the whole truth of the matter is that, were half the efficacy attributed to electricity in curing disease in other

parts of the body that is attributed to it in the treatment of the generative organs, it would justly be laughed out of sight at the first hearing. If an ovarian abscess, or a pus collection in the pelvis, is curable by electricity, so is it in any other part of the body. If a fibroid can be dispersed by the same means, let it by all means be tried in bony tumors, or in necrosis of the jaw, or in osteo-myelitis. The mysterious force that can destroy one pathological product, ought not to fail in another which is no worse. The assumed results cannot be taken as real in one set of cases more than in the other. Then again the electrical treatment is set down as only suitable for a certain class of cases. What this class is no one has yet been able to ascertain. If it is fit for one set only, who is to determine when a tumor falls within this class? This is the difficulty that only the electricians are able to solve. They are able to do so, because they have never had actual expetience in dealing by sight and touch with these tumors. No man, however skilful he may be, can say positively what are the peculiarities of an intra-abdominal growth. The same is true of a majority of the mixed pelvic conditions. No lesion is unique, or as a rule simple. Complication is the rule, and where complications exist, conditions are more apt to exist in which experimentation will do harm rather than good.

Even for direct surgical operation discriminating care is to be taken in determining proper cases. It is madness to operate upon dying patients, for the result is only quoted as a failure for the surgeon and a reproach to surgery, and is an alarm to other sufferers. In acute inflammatory troubles, septic or specific, it is sometimes urged to wait until adhesions have formed, or until pre-operative treatment has been applied, to simplify the operation. This has no justification in experience. All the preparatory treatment advisable is that necessary to prepare the patient, so far as her ordinary functions are concerned, to undergo the stress of operation. Stimulation and tonics are at times demanded, as well as measures to quiet an irritable stomach. When lung complications exist, operation is to be deferred until these are held in abeyance. Except in suppurative disease it should also be well assured that the kidneys are not doing defective work. After operation where there is the least suspicion of kidney derangement opium is all the more to be avoided, not only as useless but as harmful. Now while early operation is to be advocated in inflammatory conditions, in evident pathological changes and in neoplasms, it is the more to be urged here, because here there is no hope of effecting resolution, for the trouble is organic from the start. Waiting increases the size of the growth and its adhesions interfere with other organs. There is no better proof of this than the result of present operations, in which it is the rule for tumors to be small at removal, compared with the results of the same class of operations a decade ago. The figures are all on the side of prompt removal when a foreign growth is discovered.

Complications are the chief difficulties of all pelvic and abdominal operations. The removal of a simple cyst, or of a growth, unconnected with other organs, is usually simple, but when there is a general fusion of all the pelvic contents, bladder, uterus, omentum and intestine in one conglomerate mass, the manipulation can be accomplished only by skilled hands made so by experience and study, and by a general knowledge of normal relations, and of those assumed under pathological conditions.

A knowledge of how to deal quickly and safely with adhesions, requires more special abdominal training than can be understood or appreciated by the novice or the prurient thirster after abdominal fame. Men of this sort are rendered more dangerous than rabid animals if they by chance in their first encounter with abdominal or pelvic surgery meet an easy case. They then imagine they have mastered the whole subject, and are accordingly all the more unfit to meet the exigencies of any particularly difficult case, because they underestimate the possibility of complications and overestimate their own ability. Of late there has been a great mass of discussion on the sequelæ of abdominal surgery. This discussion involves both the existence and the causation of the sequelæ. Too often the occurrence of some particular sequel has been explained from a personal not a scientific standpoint. Fistulæ for instance, have been attributed to the use of the drainage-tube, when a close study of the operator's work shows that he does not know how to care of a drainage-tube, and therefore is not competent to either commend or condemn it. To clean a tube once in twenty-fours is to put it where it is not needed, and the same is true of cleaning it every twelve hours. A drainage-tube carefully introduced, carefully kept clean, and early removed, has no more reason to cause a fistula, than has the suture in the uterus after Cæsarean section. Another class of operators trace their fistulæ to ligatures, silk being especially condemned. Now I have never, so far as I know, had a fistula after operating, while I have used silk constantly for the stump and the pedicle. The great points are to have as fine a ligature as is consistent with safe tying, to tie in sound tissue, and to cut the ligatures close. Catgut I regard as more or less unsafe, both from its nature and from its physical properties. In the first place we cannot know that it is really aseptic, and secondly it shrinks on the pedicle. Mann, in a late report of

his work, lays the blame of fistulæ on the silk ligature and then, strangely, reports a case where a gut ligature had given him identical trouble. Such statements only show that the faultiness of certain procedures, more than anything else, lies at the bottom of the occurrence of this complication. As to hernia, I have elsewhere dealt with that subject so fully, both in papers and discussions, that I do not deem it proper to resume it here.

Finally, the results of all these operations depend largely on the experience and training of the operator. Many cases do not necessarily make a skilful operator. Many men of large experience, so far as the number of operations is concerned, do not obtain results such as we would expect. Other men with a comparatively few cases have results that stamp their work as careful and conscientious. Work in order to be successful must be done only for the good of the patient, without thought of self or of grafting original methods or instruments upon a branch of surgery already too much cumbered with names and paraphernalia. The trappings, the adornments, and the cumbersome machinery of the old operating-room and table are all to be set aside; simplicity and directness are to be sought, and above all a learning, receptive spirit, that is not vain or puffed up with its own accomplishments, and that learns from others and by its own failures—for failures we all must have. They should be few, but they nevertheless will occur. Miracles are no more possible in this branch of surgery than in others. Too much must not be promised, and too much will not be expected. No man can have good results who is not content to train himself faithfully, carefully, diligently, and tediously. To fail in these latter days of the art, because we do not know how to tie a ligature or to clean a drainage-tube, or to stitch and resect an intestine, is unjustifiable homicide.

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## *A PLEA FOR THE GENERAL ADOPTION OF THE AXIS-TRACTION FORCEPS.*[1]

BY JOSEPH HOFFMAN, M.D.,
OF PHILADELPHIA.

IN presenting any question concerning the forceps I am well aware that I am treading upon hackneyed ground. Without preface I shall hasten to the statement of my argument for the adoption of the axis-traction forceps. First of all, the use of the forceps is to be considered as an obstetric procedure, which before its employment is to be fully justified, both in the interests of the mother and of the child, entirely apart from the convenience or prejudice of the operator. The forceps are to be applied, neither because the mother demands them nor because they are a time-saving convenience for the obstetrician. Their use is strictly a logical one, and they must be indicated or contra-indicated, be differentiated from every fallacy of apparent justification. The question has been asked, Can there be a too frequent use of the obstetric forceps? On the other hand, we frequently hear of the "abuse" of the instrument. Now, if some practising obstetricians are in doubt as to whether they shall use the forceps on every occasion where there is delay or other seeming cause, and if others only occasionally use them, the position of the two is evidently entirely different. If the application is for obstetric good and not for convenience, which must be conceded, they must save either mother or child, or both, from troubles or dangers they would otherwise undergo. Given a normal pelvis and a normal head and presentation, with normal application of forces, the labor has only to produce the dilatation of the maternal soft parts. If such dilatation, both perineal and cervical, is tardy, it is evident that an early application of the forceps must be accompanied with injury to the maternal structures, while the injury to the child must, or should, be small, under skilful application of the instrument. If now, on the other hand, there is free relaxation of all the soft parts and a deviated presentation, or a slight disproportion between the diameters of the fœtal head and those of the pelvis, which uterine contraction and external manipulation have been unable to overcome, thereby prostrating the mother and interfering with the fœtal circulation, the use of the obstetric forceps comes under consideration as a preservative measure, and is directly advantageous, as the instrument can be applied without force or forcible effects upon either mother or child. It is at this point that the general misconception of the use and intention of the forceps arises. Probably it is a teaching fault as well as a learning fault. Once let it be considered that the fœtus in the pelvis is a foreign body like the aching tooth in the jaw, and we have the obstetric forceps and the old tooth-key, as similar instruments of torture, both having force as the foundation of the ends they would attain. This principle, unfortunately, even if not confessed, is that followed out by too many obstetricians and obstetrical teachers, whose lectures must indorse, as a result of their ideas, pressure forceps, blades useful for leverage, and the like. This idea makes the forceps a lever or a tongs, with which to pull or pry by force, and loses sight of the principle that it should be used as the supplement of existing forces and in their lines if they are normal, to restore them if abnormal, and to conserve them or reinforce them if they are weak, and to substitute them if under certain conditions

[1] Read before the American Medical Association, Section of Obstetrics, May 22, 1890.

they are absent. Once accept and apply these principles and the abuse of the forceps ceases, even in cases where otherwise their application would be questionable. So far as the fœtal head is concerned, in what light must the ordinary obstetrical forceps be considered? From a mechanical standpoint we must see that the instrument works at a manifest disadvantage. Its adaptation to the fœtal head is maintained, no matter what the resistance opposed by the maternal structures, by a maximum pressure at the extremity of the long arm of the lever composing the handles. If the resistance is small the pressure is not less, but only shorter, owing to the speedy escape of the head, and expends itself to the disadvantage of the mother, and with damage to the perineal floor. If this pressure is not applied the instrument slips, and damages both the fœtal head and the maternal structures. If the resistance is great, the compressing force must be necessarily great to overcome it, while the contusing force upon the maternal structures is proportionate to the force expended in the axis of the pelvis and that lost against the pubic bones. In the use of the ordinary forceps this must exist; it is a mechanical necessity and must be overcome, if at all, by mechanical principles efficient according as they approach mechanical exactitude. True mechanics teaches us to use force gently, and if this is done better results are reached. Considered from the standpoint of the fœtus the force expended against the pubis is lost; considered from a maternal standpoint the force becomes harmful in its pressure-effects, just as the additional force by direct pressure upon the fœtal head is also harmful. It therefore appears that considered mechanically the ordinary obstetrical forceps is a defective instrument, permitting neither the satisfactory utilization of the force applied, nor its regulation so as to conform with the principles previously suggested. Now, as the pelvic axis is the line in which all forces must act in order to be effective, it follows that the greater the deviation from a normally formed pelvis, the greater must be the resistance to any force acting outside of or at variance with this axis, and that in cases of pelvic malformation the violence must be correspondingly great, both to maternal and fœtal structures.

The problem is accordingly presented as follows: Given a resistance; we must apply a force which shall work along the axis of the pelvis, and at the same time minimize lost energy and maximize the energy exerted in the axis of the pelvis and which alone is efficient in the expulsion or delivery of the fœtal head. The more nearly this problem is solved the more direct will be the traction, the less will be the force required, and the more natural will be the artificial delivery, or, as has been suggested above, the two will supplement each other. To fulfil the above indications numerous modifications of the common forceps have been invented, among which are those of Hermann, Hubert, Hartmann, Morales, and these, I think, we may justly say, finally culminated in those of Tarnier. The latter have been essentially modified by Poullet and others. The latest German idea, that of Breus, I regret not to be able to present. It is ably advocated by Winckel, though perhaps too much to the exclusion of Tarnier's forceps.

The conformity of the pelvis is such that when occupied by the fœtal head the problem is to bring an ovoid body through a circular canal in which a curved axis is presented. Now, to accomplish this it is evident that the force should be applied to the centre of the head or fœtal ball, and should act in the centre of the cylindrical passage, the vagina, at the same time varying the position of the application of the force as the head descends in the pelvic curve.

The ordinary objection urged against the Tarnier forceps is the danger of rupturing the perineum by the traction apparatus. This is entirely theoretical, as the rods are to be kept parallel with the forceps handle, and when this is done they practically fall within the blades, and can do no damage that would not otherwise occur. With both Tarnier's forceps and Poullet's modification some of the blunders yet common with some practitioners cannot be perpetrated. I refer more particularly to the so-called pendulum or lever motion of the forceps, and to the rotatory motion advocated by Osiander. These manipulations, while proper in the ruder arts, have no place in obstetrics, and can result only disastrously to the maternal tissues. It will be also seen that the direction taken by the head as it engages, must be in the path of the least resistance, inasmuch as the traction exerted does not guide, but only applies a force which is directed by the handle, which indicates lateral variations in the descent upon the perineum. These lateral deviations are the foundation of the excuse for the pendulum movements, which are only experimental manœuvres to determine in what direction the head will move most easily.

A brief epitome of the specific positions in which axis traction is especially advisable is all that can be here set down. First of all, in high operations in which it is desirable to make traction in the pelvic axis and not against the brim, a careful analysis of cases will show that the axis-traction principle offers better results so far as the mortality of the child and the effects on the maternal structures are concerned than pressure forceps.

As was before remarked, in deformed pelves the results must be even more notable. In occipito-

posterior positions any tendency of the head to rotate is not interfered with, but is allowed to take place freely. In my hands I have noticed a complete revolution of the blades and, of course, of the head in their grasp. In other occipito-posterior positions, in which the perineum usually suffers severe laceration, the lifting effect of these forceps —which, while bringing the head downward and outward, nevertheless raises it toward the pubes—is peculiar, and cannot be imitated by any other procedure. The rotary motion of Osiander, while essentially the same in its intent, cannot be compared in its accomplishment.

In the low operation, in which the short forceps are commonly applied, the same effect can be justly claimed. The short forceps, as such, in my estimation have no place in obstetrics; I have no doubt that of all instruments they inflict the most damage to both mother and child. If, when the head is on the floor of the pelvis, there is any need for the application of forceps, the axis-traction principle is the one that fulfils all the indications, expediting labor and at the same time saving the perineum by lifting, not dragging, the head through.

The final argument for the general adoption of the axis-traction principle is, that by it the force is applied at the centre of or near the centre of the fenestration, and that hence the leverage effect which must be exerted upon the structures within the grasp of the blades must cause less direct violence.

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## THE CONTINUED FEVERS OF THE SOUTH.[1]

BY W. W. JOHNSTON, M.D.,
OF WASHINGTON, D. C.

THE opportunity of discussing the subject of the "Continued Fevers of the South" with men who are constantly and carefully observing them is one which I appreciate, for at the present moment there is no subject in the whole field of medical pathology which seems to me so interesting and in the study of which so much is to be learned and so great an advance to be made. The places in pathology of the prevailing continued fevers of certain parts of the country have been more or less definitely fixed within the last decade; but in the South—including under that term the Southern Atlantic States, the States of the Gulf Coast, and the Southwestern States—the difficulties in classification are much greater than in the Eastern, Middle, and Western States.

In the Southern country the conditions of climate and soil have impressed such modifications upon the prevailing forms of fever, or have created such varieties, that definition and differentiation are attended with more difficulties than elsewhere. Moreover, the continued fevers of the South are, in great part, mild in type and have a low mortality, so that, in those fevers which are most difficult to classify, the opportunity is not often afforded to study their lesions and thus to discover their true nature.

The questions to be considered, in order to reach clear views as to the nature of the Southern fevers, are these:

I. To what extent does typical enteric fever prevail throughout the South?

II. To what class do the negative, atypical forms of fever belong?

III. Has continued malarial fever or malarial fever of remittent type a very general prevalence?

IV. Is the enteric fever of the South modified by malarial infection, giving rise to a hybrid disease—typho-malarial fever?

I. In answer to the question as to whether the enteric fever of the South conforms in its type to the form of enteric fever as originally described by Louis, it may be said that among the physicians of the South the prevailing opinion is that typical enteric fever is a disease of rare occurrence, as compared with other continued fevers, and I might quote here statements of many physicians scattered throughout this region to show that "text-book typhoid" is in some places unknown, in others rarely seen. This is more true of the Gulf Coast and Southwestern regions than of the States of the Southern Atlantic Coast, where the typical form is more commonly observed. In the Gulf States the actual prevalence is less, and the death-rate lower, than elsewhere; in Texas, Arkansas, Kansas, lower Missouri, Kentucky, and Tennessee, while there are differences of opinion as to what degree of typical manifestation justifies the diagnosis of typhoid fever, yet the preponderating opinion is that the combination of true typhoid symptoms is not frequently observed. I have not been able to learn how far the present belief agrees with that of twenty-five or more years ago, but many physicians of the South assert that enteric fever is assuming a milder type and that it is becoming a rarer disease than formerly. Whether the disease is disappearing or is only putting on a new dress it is of importance to know. The latter is what my own experience teaches me is the case, for while the more virulent forms are less numerous than formerly, the disease under a milder and less fatal form is becoming our endemic summer and autumn fever, with a wide prevalence and low mortality. There are geographical differences in enteric fever, and there are also historical differences; and just as malarial fevers give place to enteric fever as cities are built and sewered, and as areas of country are more carefully cultivated and drained, so enteric fever is modified in time by changes in the soil and

[1] Read before the American Medical Association, May 21, 1890.

in other conditions of environment under which its germs grow and are disseminated.

II. To what class do the *atypical* forms of fever belong—those which do not have the typical symptoms of malarial nor of typhoid fever? This category does not include those cases of mild enteric fever which have at least one of the more characteristic symptoms of the disease in addition to the more or less typical fever curve, but only those cases which have neither rose-spots, delirium, tympanites, nor diarrhœa, but in which a continued fever of moderate elevation, with diurnal remissions and exacerbations, lasts for from two to four weeks or longer, usually ending in recovery, but terminating sometimes fatally by intestinal perforation or hæmorrhage. These fevers have often a sudden onset, with a chill, and in the initial stages the symptoms have some malarial features, but they are not shortened nor markedly influenced by quinine.

There is a growing belief that such negative forms of continued fever belong to the same class as the more typical enteric disease, the reasons for this conclusion being based upon the study of large groups of cases, which show a gradual diminution in intensity and in the number of associated symptoms, until a continued fever, more or less conforming to the type of enteric fever, is the only symptom left. The occasional death by perforation or hæmorrhage and the finding of the typical lesions in the most negative cases have gone far toward confirming this conclusion.

We recognize in the whole class of infectious disease the greatest differences in intensity of manifestation. Some cases of scarlet fever and diphtheria escape observation altogether until dropsy or paralysis makes clear the nature of a preceding, almost unnoticed illness. And so it is not strange that enteric fever should sometimes be so mild as to be difficult of recognition. The question to be answered is this: Is it more probable that these mild forms of fever are "simple continued fever," "gastric fever," or "catarrhal fever," or are they mild manifestations of typhoid infection? The latter doctrine is the safer one in practice, and will, if adopted, avert that rude awakening to the fact that what was thought a slight illness is accompanied with a specific intestinal ulceration and all its attendant dangers.

III. Has the continued malarial fever or malarial remittent fever a very general prevalence in the South?

I include under this question all those forms of continued fever which are supposed to be due to malarial influence and variously denominated "malarial continued," "bilious remittent," "malarial remittent," or "remittent" fever.

There can be no doubt, if the large amount of testimony on this point is taken as a basis for a conclusion, that there is a form of fever prevailing in the South, which lasts from five days to two weeks, which begins with a chill, followed by rapid rise of temperature, by thirst, nausea, vomiting, frontal headache, and lumbar or other pains. A remission follows in from six to twelve hours, with relief to headache and other symptoms. If not promptly treated by quinine there is a recurrence of the rigors, fever, etc., these phenomena being repeated during five, seven, nine to twelve days. Quinine in these cases has a beneficial and curative effect. The existence of such a fever may be assumed and its malarial nature accepted.

But is there an adynamic malarial fever, or a malarial or remittent fever with typhoid symptoms? After a close reading of the descriptions given of the essential symptoms of this form it has seemed to me that there is no symptom which clearly establishes its malarial origin, but much in the history of the subject to show that very many such cases are really cases of typhoid fever. The records of cases in which the diagnosis of remittent fever was made in the early stages, but changed later to that of enteric fever, when some one characteristic symptom appeared, or cases which were called "remittent" or "malarial" until death resulted from perforation or intestinal hæmorrhage, or until an autopsy revealed the lesions of enteric fever and compelled a change in diagnosis, are scattered through hospital reports, army and navy reports of Great Britain and of the United States, and in the journals for many years. I will not be so bold as to say that there is no continued malarial fever or remittent fever, closely resembling enteric fever, in the South, but there is not a shadow of doubt that many so-called cases of "bilious fever," "remittent fever," or "continued malarial fever" are, in reality, cases of enteric fever which lack most of the typical symptoms of that disease. The change of view on this subject has been very decided in the last ten years in the cities of Boston, New York, Philadelphia, Baltimore, and Washington. Here "remittent fever" and "continued malarial fever" were at one time as generally recognized forms of prevailing disease as in Nashville, Memphis, or Mobile; but at the present time few in these former cities would venture to call a continued fever lasting over ten days "malarial continued" or "remittent," no matter how great the morning remissions might be.

IV. Is the enteric fever of the South modified by malarial influence, producing a hybrid disease—the so-called typho-malarial fever?

I approach this part of the subject with much hesitation, knowing how difficulties crowd the road to its solution; but while waiting for the time when the microscopist will decide whether the microör-

ganisms of the two diseases are or are not coincident, much may be done by clinical observation to determine it. The great diversity of opinion alone serves to throw the gravest doubt on this belief.

The term "typho-malarial fever" has five distinct meanings in the minds of those who employ it. It means: (1) A continued malarial fever with typhoid symptoms which is not influenced by specific (quinine) treatment; or, (2) an enteric fever, with intermittent or remittent phenomena in the beginning or at the end of its course; or, (3) a continued fever, with gastro-hepatic disturbance; or, (4) a prolonged fever, of a certain gravity, without the typical symptoms of enteric fever, and not influenced by quinine; or, (5) a fever of a negative and mild character, without the characteristic symptoms either of malarial or of typhoid fever.

The strongest arguments for the theory of the union of the two diseases in one individual are the presence of supposed well-defined malarial symptoms in typical enteric fevers and the beneficent effects of quinine in such cases. To the first of these arguments it may be answered that the occurrence of chills in the onset or during the course of enteric fever, marked remissions in the fever-curve, gastric irritability, the vomiting of bile, icterus, and hepatic tenderness are not necessarily due to malarial infection, and may and do occur in the course of the enteric fever of localities where malaria is unknown. The most severe chills are seen in the course of enteric fever in Washington; they are probably septic in nature, and are not prevented by quinine treatment.

It is not wise to be too dogmatic on this point, but as many writers of the South have advanced such strong assertions in denial of the existence of typho-malarial fever, I feel more assurance in expressing my own adhesion to the same view. In no instance have I seen a fever which seemed to require such an interpretation, and in view of the fact that twenty-five years have passed since the hybrid name and theory were suggested by Dr. Woodward, a very large mass of testimony ought to be forthcoming in its support, and yet the medical world has not accepted the doctrine as an essential article of faith. Among those who do accept it there is such a contrariety of opinion as to what typho-malarial fever is that we may even hesitate to believe that there is any truth in the doctrine or any well-founded arguments to support it. What is needed on this question is an authoritative collective investigation, and the American Medical Association could do no better work than to order such an investigation by a competent commission which shall sift the truth out of the mass of uncertainty and give some simplicity and definiteness to our knowledge. There is no uncertainty in the facts—it is in our interpretation of them only, and a properly constituted body of investigators may be trusted to act without prejudice and to formulate conclusions which will carry great weight to all impartial minds.

The following conclusions embody the main statements in the above remarks:

1. Enteric fever is a rare disease in the South in a typical or intense form.

2. There is a probable change going on in the type of enteric fever; it is losing its typical character and is assuming a less typical and milder form.

3. Many cases of mild continued fever, which have no well-defined or characteristic symptoms, are cases of mild enteric fever.

4. While malarial continued fevers are found in the South, many so-called cases of "adynamic malarial fever," "remittent fever," etc., are in reality cases of enteric fever.

5. There is no good reason to believe that there is such a disease as typho-malarial fever.

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## CLINICAL MEMORANDA.

### OBSTETRICAL.

***Detachment and Expulsion of the Vaginal Portion of the Cervix during Labor.***—This rare accident recently happened to a patient in the Maternity Hospital:

A. S., V.-para, aged thirty-four, had a history of difficulty in previous labors, which, with one exception, had been instrumental, partly on account of a slightly contracted pelvis, but mainly because of unusually large children. Labor began by rupture of the membranes; vigorous uterine contractions soon followed; but little progress was made in spite of energetic abdominal action which the woman persisted in exerting with almost every pain. The os dilated unevenly, the anterior lip, nipped between the head and symphysis, remained thick, firm,

Vaginal portion of the cervix detached and expelled during labor.

and unyielding, but was not œdematous. After more than twelve hours of ineffectual uterine and abdominal contraction a fleshy mass was discovered protruding from the vagina; it proved to be the vaginal portion of

the cervix, lacking a small piece posteriorly. On examination the head was found well down in the pelvic cavity, surrounded by the vaginal walls. The expulsive forces then failing, the child was extracted by forceps. Twenty-four hours after labor a vaginal examination was made. No trace of a cervix could be felt except posteriorly, where a tongue of cervical tissue, about the breadth of a finger and one and a half inches long, projected into the vagina. There was no hæmorrhage during labor nor afterward, and the puerperium has been uncomplicated except for a slight cystitis. At the present time, three weeks *post partum*, the tongue of cervical tissue projects from the median line posteriorly, the entrance to the womb is reduced to its ordinary calibre by cicatricial puckering in the centre of the vaginal vault, and the involution of the uterus has progressed normally.

The dilatation of the os was, no doubt, prevented in in this case by cicatricial infiltration, the result of injuries in former labors. The absence of serious symptoms after an injury of this gravity is remarkable. It speaks well for the asepsis obtained in the Maternity Hospital.

BARTON COOKE HIRST, M.D.,
Professor of Obstetrics in the University of Pennsylvania.

***An Anencephalous Twin.***—Mrs. H., on May 8th, gave birth to male twins. The first born was well formed, vigorous and healthy, weighing at birth seven pounds. The second was anencephalic, with almost entire absence of the occipital and parietal bones. The hemispheres of the brain were only partially developed and were apparently covered by dura mater.

Both children presented by the vertex and were in separate sacs; their placentæ were united by the edges. The monster was the larger of the two, and, with the exception of the head, was well formed, weighing eight and one-half pounds. The mother ceased to feel motion upon the right side about the beginning of the ninth month of gestation, and the fœtus probably perished about this time. The funis was shrunken and small, and the surface of the body was easily denuded of cuticle. No post-mortem was made, as I desire to present the specimen intact to the Lexington and Fayette Medical Society.

By palpation, at the beginning of labor, both children could be distinctly outlined. By auscultation the fœtal circulation could be heard upon the left side only. The abdomen presented a broad and flat appearance.

Antisepsis was carefully practised in this case, and both the patient and the living infant are doing well. This was the woman's seventh confinement and her first twin labor. Upon the subject of maternal impressions I made particular inquiries, and learned that about the beginning of the fourth month of gestation the patient had been frightened by the attempt of an intoxicated man to enter her bed-chamber. For several days after this she suffered from nervous prostration. But if the shock thus received was the cause of the abnormal development in the one fœtus, why was not some impression made upon the other? F. M. GREENE, M.D.

GREENE DALE, KY.

## MEDICAL PROGRESS.

***Antiseptic Paste.***—SOCIN recommends the following paste to be used in place of other antiseptic dressings on regions where it is difficult to apply a bandage (*Medicinische-chirurgische Rundschau*, May 15, 1890):

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| ℞.—Zinc oxide | . . . | 12½ drachms. |
| Zinc chloride | . . . | 75 grains. |
| Distilled water | . . . | 12½ drachms.—M. |

This paste should, if possible, be used when fresh. It dries quickly, forming a strong crust which can be still further strengthened by incorporating a small amount of cotton-wool with the paste.

***The Treatment of Chronic Bronchitis.***—In an instructive lecture upon bronchitis (*Lancet*, May 24, 1890) HARRIS gives the following advice on the treatment of the chronic form:

The inhalation of steam alone night and morning helps the expectoration, but the addition of compound tincture of benzoin, creasote, eucalyptus or carbolic acid in small amounts, adds considerably to the value of this method of treatment. If one drug seems too irritating, another may be tried. The patient must be induced to give this plan a fair trial, but must be directed not to employ the inhalation too long at one time, as faintness may be induced. Here should be mentioned the sprays of various medicaments recommended by Drs. Ringer and Murrell, and used by them with such success. The drug most frequently employed is ipecacuanha wine, either pure or diluted with once or twice its bulk of water. Simple spray apparatus may be used with the ipecacuanha wine, and the patients should be directed to inhale the spray as given off. At first there may be some involuntary resistance to the entrance of the drug into the lungs; but, as the patient gains confidence, it no doubt enters freely into the air vesicles. Instead of the ordinary spray apparatus a steam atomizer may be used. Dr. Murrell claims that with a few sittings, at each of which two drachms or more of ipecacuanha wine are used, the patients are greatly relieved, the shortness of breath and cough are diminished, and good rest at night is obtained. The number of inhalations required in most cases of chronic bronchitis ranges from four to eighteen, and they should be given daily. The effect of ipecacuanha is said to be increase of the secretion. In small doses it depresses blood pressure but little, while it stimulates ciliary action and that of the respiratory centres. Tartar emetic spray (one grain to an ounce of water) has also been used; it produces the same effect as the ipecacuanha wine, but is less satisfactory. Lobelia spray is sometimes employed, especially when there is much spasm. Iodide of potassium spray (two per cent. solution) is also of much use.

Counter-irritants seem to be of great value in chronic bronchitis, and the author has great faith in the linimentum terebinthinæ aceticum (B. P.) well rubbed into the chest night and morning. Others use linimentum iodii, with the idea that it is not only an active counter-irritant, but that it also does good when inhaled, iodine having been strongly recommended as an inhalation in bronchitis.

As regards expectorant and other drugs in the treatment of chronic bronchitis Harris gives the preference to carbonate of ammonium, five grains of the salt with infusion of senega or serpentaria, or with aromatic spirit of ammonia, ipecacuanha wine and compound tincture

of camphor. Chloride of ammonium is also of much service. Terebene, in the author's experience, gives relief at first, but he has not found it of as much value as carbonate of ammonium. Emetics are often very serviceable in cases with excessive secretion which is not easily expectorated.

***Treatment of Profuse Menstruation.***—The following prescription for cases of profuse menstruation is quoted by the *American Practitioner and News:*

℞.—Dialyzed ergotin . . . 10 drachms.
Glycerin . . . . 5 "
Salicylic acid . . . . 30 grains.
Distilled water . . . 2½ ounces.—M.

One teaspoonful diluted with three teaspoonfuls of water to be injected into the rectum after stool once daily.

***Bougies Weighted with Mercury.***—DR. LEOPOLD CASPER uses elastic bougies filled with mercury which he thinks have decided advantages over both metallic and ordinary gum bougies. The weight of the mercury causes them to slip into the bladder with great ease and they possess all the safety of the unweighted elastic bougies.—*Therapeutische Monatshefte*, May, 1890.

***The Treatment of Local and of General Peritonitis.***—DR. W. E. B. DAVIS, of Birmingham, Alabama, considers that the following facts in reference to peritonitis are definitely settled:

1. Simple peritonitis, when caused by a sufficient quantity of chemical irritant, will produce death by the extent of the inflammation.

2. Simple inflammation may terminate in septic peritonitis, by producing a weakened condition of the walls of the intestines, which permit the passage of septic germs from the intestinal canal into the peritoneal cavity.

3. While pathological germs in small quantity may be absorbed by the healthy peritoneum, without producing a peritonitis, the same quantity combined with a chemical irritant may produce a violent inflammation—the irritant having prevented the absorption of the germs and caused the exudation of a nutrient fluid for their multiplication.

4. Large quantities of septic fluids and microbes always produce suppurative peritonitis; yet, a small quantity of either may be absorbed and destroyed, unless the peritoneum has been weakened by antecedent pathological changes.

5. A septic fluid may gravitate into dependent parts of the peritoneum, and become shut up, either by plastic inflammation, or by a coil of intestine, and thus be prevented from producing diffuse peritonitis, but after a time this may rupture and produce death from general peritonitis.

6. The germs of septic peritonitis will be found in the kidneys and other organs of the body, and in quantities proportionate to the extent and duration of the inflammation.

7. The condition of the peritoneum, and the nature and quantity of the product, will determine the rapidity of the inflammation, which usually ends in from forty-eight hours to six days, but death may be produced from shock in a few hours. Tubercular inflammation is always slow in its progress.

The foregoing principles indicate the following rules of treatment:

(*a*) Promote absorption of the inflammatory products of simple peritonitis as rapidly as possible, and thus relieve the inflammation and prevent the possibility of septic peritonitis.

(*b*) In the early stages of peritonitis, whether simple or septic, where the cause cannot be determined, hasten the absorption of inflammatory products, etc., with purgatives.

(*c*) When medical treatment fails to give relief, septic fluids should be removed by operative procedure.

(*d*) In localized peritonitis, with circumscribed pus formation, the pus should be removed and the abscess cavity drained.

(*e*) In acute septic peritonitis, operative procedure must be adopted early, or there will be no chance of recovery offered by the operation, as the inflammation will become more extensive the longer it continues; and, too, there will be so great a quantity of septic germs absorbed into the system that death will result from toxæmia, even though the local inflammation should be remedied by a late operation.—*Virginia Medical Monthly*, May, 1890.

***Ammonium Bromide Fumes in Asthma.***—The value of the fumes of ammonium chloride in some varieties of bronchial and naso-pharyngeal catarrh suggested to DR. THEODORE MAXWELL the use of ammonium bromide fumes in the same diseases when complicated with spasm, as is seen in asthma (*Lancet*, May 10, 1890). The ordinary solutions of hydrobromic acid were found useless for the purpose and Dr. Maxwell was obliged to use a strong acid with a specific gravity of 1.7. This, with solution of ammonia, gave abundant fumes. In several patients suffering from asthma a few whiffs relieved the dyspnœa at once. In some cases when used early the inhalations aborted the paroxysms.

As some difficulty in obtaining absolutely neutral fumes with the usual inhalers was experienced, an apparatus was finally adopted in which the ammonium bromide is simply vaporized by heat and drawn through a wash-bottle before being inhaled.

***The Treatment of Dysentery in Children.***—VEILLARD recommends the following mixture in the dysentery of children:

℞.—Powdered ipecacuanha . . 25 grains.

Boil for five minutes in 3½ ounces of water. Filter and add:

Tincture of opium, from . 2 to 4 drops.
Cinnamon water . . . 3 drachms.
Syrup of orange flowers . . 6 " —M.

Dose, for a three-year-old child, one dessertspoonful every hour, or at longer intervals if nausea is produced. To quiet tenesmus enemata containing tincture of opium, or enemata of infusion of chamomile or of eucalyptus flowers, should be used.—*Annals of Gynæcology and Pædiatry*, May, 1890.

***Pilocarpine in Deafness.***—MR. GEORGE P. FIELD reports a series of eighteen cases, diagnosed as labyrinthine

deafness, treated with pilocarpin. In all of these cases the improvement was marked. The cases which derived the greatest benefit from the treatment were those in which bone conduction was the least. A few cases of middle-ear deafness were also somewhat improved.

The author states that the pilocarpine was given hypodermically but does not state the dose nor the frequency of its administration.

***Prescription for the Expulsion of Lumbricoid Worms.***—According to the *Annals of Gynæcology and Pædiatry* the following is DUJARDIN-BEAUMETZ'S prescription for the treatment of lumbricoid worms:

℞.—Tincture of kameela . . 1½ drachms.
Syrup of orange-peel . . ½ ounce.
Water, sufficient to make . 4 ounces.—M.

This should be taken in small doses at short intervals until all has been used. If the worms are not expelled within two hours after taking the last dose castor oil should be administered.

***Formulæ for the Administration of Condurango.***—KRAUS, of Vienna, who uses condurango as a stomachic gives it in the following combinations:

℞.—Extract of condurango . . 30 minims.
Chloral . . . . . 15 grains.
Syrup of orange . . . 5 drachms.
Distilled water . . . 4½ ounces—.M.
Dose, from three to six tablespoonfuls daily.

Or,

Condurango bark . . 5 ounces.
Hydrochloric acid . . 15 minims.
Elixir of orange . . . 2½ drachms.
Syrup . . . . . 5 ounces.—M.
One tablespoonful every two hours.—*Internationale klinische Rundschau.*

***Quadrangular Sounds for the Treatment of Urethral Stricture.***—DR. HAL C. WYMAN has devised a four-sided bougie, with a groove between each angle, which he thinks has several points of superiority over the ordinary instrument (*American Lancet*, May, 1890). A round bougie in passing a tight stricture often severely stretches the urethra longitudinally and not infrequently causes laceration, whereas the quadrangular bougie, presenting less surface for friction, slips through more easily, at the same time dilating the stricture. The grooves between the angles insure the presence of the lubricant where it is most needed, and, moreover, by means of them medicaments can be applied to the urethra.

***Asepsis of Urethral Instruments.***—PICARD (*Gazette des Hôpitaux*, No. 28, 1890) writes that a clean aseptic bougie or catheter, no matter how often employed, never occasions cystitis, urethritis, orchitis, urethral abscess, or urinary fever. To this should be added—provided the urinary tract be not previously infected.

Respecting the means to be employed to secure asepsis, the author confines himself to those easily adopted by the general practitioner. Metallic instruments are most easily disinfected. Boiling in simple water or in salt water is regarded as giving practically the necessary guarantees. Boiling in strong carbolized water is preferable, but often impracticable. Soft or semi-soft instruments require greater care. The soft, red, Nélaton catheter is easily disinfected by dipping for a few seconds into boiling water, or it may be kept in a strong antiseptic solution, such as a five per cent. carbolic acid or a 1-to-1000 sublimate solution. It is recommended to wash the instrument either in boiling water or boric acid solution before using. After boiling, the instrument may be taken up with clean forceps and placed in a jar containing a saturated solution of boric acid. A thread previously fastened to the end of the sound is slipped into a slit in the cork, and the sound thus suspended can be readily withdrawn without handling when needed. Semi-soft catheters and bougies are not so easily sterilized. They should be placed in tubes closed with absorbent cotton. The tube is then placed in boiling water and allowed to remain for half an hour. During this time this heat will sufficiently sterilize the tubes. It will be more prudent to repeat this process several times at intervals of twenty-four hours, for while this temperature destroys bacteria it is without effect upon the spores.

Before passing a bougie or catheter the hands of the physician should be thoroughly cleansed and rendered aseptic. The meatus and glans penis of the patient should be washed with a sublimate solution or with hot water. If the urethra is already infected it should be washed out with a saturated solution of boric acid. For lack of these precautions a more or less acute cystitis has been again and again set up.—*Journal of Cutaneous and Genito-urinary Diseases*, May, 1890.

***Drainage Water from Cemeteries.***—DR. E. VON ESMARCH (*Zeitschrift für Hygiene*) has examined the question of the dangers of contagion through the drainage from cemeteries. His experiments show that where bodies are placed in water or buried, the earth or water surrounding the bodies does not become contaminated with germs. He finds that in the case of death from anthrax the bacilli in the body very quickly die. He concludes, with several other German observers, that the proximity of cemeteries and the drainage water from them have no influence in the spread of epidemic or contagious diseases. He unfortunately does not tell us whether the typhoid bacillus meets with the same fate as that of anthrax.—*Brooklyn Medical Journal*, June, 1890.

***Filaria Sanguinis Hominis.***—At a recent Intercolonial Medical Congress of Australia DR. BANCROFT announced some of the results of his studies of filaria. He thinks that probably all filaria, whose embryos circulate in the blood, require for their distribution the intervention of some blood-sucking animal, such as the mosquito, sand fly, louse or flea, all of which should be studied. The larva of filaria immitis are often found in dogs, and the embryos are found in the lice infesting such dog.

Dr. Bancroft was the first to discover filaria sanguinis hominis in Australia (1876). The filariæ were found in an abscess of the arm in a butcher's boy, embryos being found in the blood. Since then other cases have been observed. The author thinks that one of the modes in which the parasite may prove injurious is by invading the heart and causing thrombosis. They may also produce emboli.—*Medical Chronicle*, June, 1890.

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## THE INFLUENCE OF MATERNAL HÆMORRHAGES UPON THE VITALITY OF THE FŒTUS.

MANY investigators have studied the effect of certain maternal conditions upon the life of the fœtus, and both experiments and clinical observations have established some facts of great practical importance. Thus, it has been shown that a sudden elevation of temperature to 105°–106° is usually fatal to fœtal life, while even 108° gradually reached has been apparently harmless. The ill effects of emotion and maternal impressions have often been disputed, but an interesting paper by Fordyce Barker would seem to establish the fact that profound maternal impressions in some way influence the growth and development of the fœtus. The danger to fœtal life of poisons in the maternal blood has been carefully investigated, and no one denies that eclampsia, lead-poisoning, the saturation of the blood with bile salts, and infectious diseases in the mother frequently cause the death of the fœtus. It is also well known that death of the fœtus is very probable when the mother is affected with any chronic disease rendering her nutrition so defective as to induce a condition of marked anæmia. It is in this way that the pernicious vomiting of pregnancy destroys the vitality of the fœtus.

At a recent meeting of the Société de Biologie, MM. CHARPENTIER and BUTTE reported the result of a series of experiments made to determine the effect of hæmorrhage of the mother upon the vitality of the fœtus. Gravid rabbits, near term, were immersed in a six-tenths of one per cent. saline solution, the abdomen and uterus being opened in such a way that the condition of the fœtus and the umbilical circulation could be observed. The rabbits were then bled, and the results led to the following conclusions: When the hæmorrhage is sudden and profuse enough quickly to cause the death of the mother the fœtus will survive if it is immediately extracted. If the loss of blood is gradual, yet sufficiently great to cause death eventually, the fœtus will die a short time before the mother. Finally, if the mother survive a severe hæmorrhage the fœtus will die about an hour after the appearance of the hæmorrhage.

These observers attribute the death of the fœtus before that of the mother to the disappearance of a part of the oxygen of the maternal blood, to the diminution of the total amount of oxy-hæmoglobin, and also to the diminished blood-pressure which impairs the umbilical circulation. The supply of oxygen to the fœtal structure is thus insufficient for the purpose of carrying on the functions. These experiments are of practical value and offer suggestions which may be applied to cases not infrequently met with, and will, no doubt, lead to the saving of life. Examples of serious hæmatemesis, hæmoptysis, and epistaxis in the latter months of pregnancy are by no means rare, and the further history of such cases is the birth some weeks later of a macerated fœtus which has evidently been dead for some time. It would seem from these experiments that the death of the fœtus in such cases is directly dependent upon the hæmorrhage of the mother. If this be the case in obstetric practice the indication is plain to induce labor as soon as such a hæmorrhage makes its appearance, with the purpose of saving the child's life. Since the introduction of incubation and *gavage* in the management of premature infants, and the happy results of their employment—twenty-two per cent. of the infants saved at six months, eighty-nine per cent. at eight months—this plan of extracting the child when a severe hæmorrhage has occurred after the sixth month would appear both desirable and justifiable.

## THE MICRO-BIOLOGY OF RHEUMATISM.

IN *La Médecine Moderne* of May 21, 1890, is a communication by DR. BORDAS upon some micro-biological researches on acute articular rheumatism,

which while they cannot be considered absolutely conclusive are nevertheless exceedingly interesting as an earnest of what further investigation in this line may bring forth. It is but a few years since malarial fever was a reproach to the practical physician in that he could not state the manner in which this disease was cured by quinine. Through the discovery of the malarial germ of Laveran and the additional knowledge that quinine exercises a germicidal effect upon this organism, this stain upon the fair name of rational medicine has been removed, and the contribution of Bordas is important as forming what may perhaps be a prelude to the removal of the other reproach to medicine, viz., the cause of rheumatism and the manner in which salicylic acid does good in this disease. This is not the place to detail all the experiments of the investigator whom we have named, and we can do no more than give his conclusions, which are as follows:

He asserts that the bacillus which he has isolated, when injected into the carotid artery of a rabbit, produces endocarditis with vegetations upon the valves.

That acute articular rheumatism with its complications may be considered as a disease produced by microbes analogous in their production, for example, to the micrococcus pyogenes.

This investigator is also certain that the germ which he has isolated is a specific one. The research was carried out under the directions of Professor Germain-Sée, and if further studies confirm the ones just quoted, another advance in the study of disease will have been made.

## REVIEWS.

REPORT OF MEDICAL EDUCATION, MEDICAL COLLEGES, AND THE REGULATION OF THE PRACTICE OF MEDICINE IN THE UNITED STATES AND CANADA, 1765 to 1890. By JOHN H. RAUCH, Secretary of the Illinois State Board of Health. Springfield, Illinois, 1890.

WE are glad to notice in the columns of THE MEDICAL NEWS the appearance once more of the publication issued by the Illinois State Board of Health upon this most important subject. Elsewhere the writer of this review has alluded in terms of the highest praise to the work which Dr. Rauch and the rest of his Committee are constantly performing.

Although we are, as a nation, proud of the boast that one State can have but little influence upon the United States, we have in this report an evidence that by proper legislation one State can influence the educational centres not only of the United States but also of Canada.

The enormous amount of labor required in the compilation of the statistics given us by the distinguished statistician who edits these reports, can be conceived of by no one who does not thoroughly examine its contents. The State of Illinois puts into the hands of every regular practitioner of medicine the means by which he can stop the practice of any illegal practitioner at any time through the discovery of the standing of the school from which he claims to be a graduate, and puts every man, woman, and child under a debt of gratitude for its influence upon medical education on this continent.

## CORRESPONDENCE.

### LONDON.

*The Recent Epidemic of Influenza. The Relation of Salicylic Acid to Uric Acid and Acute Rheumatism. The London Post-graduate Course.*

*To the Editor of* THE MEDICAL NEWS,

SIR: The epidemic of influenza being now virtually at an end, it may not be uninteresting to glance briefly at some of the facts in connection with it. The main effects of the outbreak were felt during the first two weeks in January, but the disease was in full swing during the entire month as well as during the first week in February. It is quite impossible to obtain acurate statistics for the whole metropolis, but some idea of the number attacked may be gained by reference to the statistics of St. Bartholomew's Hospital; I must premise, however, that there is some reason for thinking that more cases were met with in this hospital than in others, possibly owing to its situation. During the first two weeks of January the number of medical patients was 3612 and 3397 respectively, and the weekly average for the whole month was 2682, the average of the preceding five years being 1530; it was estimated that about two patients out of three, or at least considerably more than one-half, were suffering from influenza. It was also estimated that during the three weeks of prevalence over 6000 cases were treated at this hospital. On the other hand, at the Hospital for Sick Children, Great Ormond Street, which is only about a mile distant, not half a dozen cases occurred amongst the in-patients, and only an inappreciable number amongst the out-patients.

In the General Post-office, where some 13,000 persons are employed, and where the average daily sick list is 250, on one day in January there were no less than 2000 absentees, and this would not necessarily represent every case of the disease, as those who suffered from the milder forms doubtless continued at work. It has been roughly estimated that in London alone no less than 400,000 persons were attacked, an estimate which certainly has not erred on the side of exaggeration.

During the first five weeks of the year the deaths in London exceeded the average number by over 2000, and 378 deaths were directly ascribed to influenza, respiratory diseases being credited with nearly the whole of the remaining excess. Of course in calculations of this kind no account is taken of the minor, but not necessarily trifling effects of influenza, and the number of those who have been left with permanently impaired health must be very considerable. After all, the death-rate in London has remained considerably below that of

the other principal cities of Europe, so we have not much to complain of.

Dr. Alexander Haig, who has already done much good work in regard to uric acid, has just read a paper before the Royal Medical and Chirurgical Society on the relative effects of salicin and salicylate of sodium in the excretion of uric acid and their value in acute rheumatism. He finds that salicylate of sodium has about thirteen times the excretive power of salicin, and that their action in disease appears to correspond roughly with their action on uric acid secretion. According to him, all methods of treatment that are of any value in acute rheumatism cause an increased excretion of uric acid, and salicin and the salicylates are useful in this disease in exact proportion to their power of producing elimination of this substance. He then proceeds to give reasons for thinking that acute rheumatism is due to uric acid, and he expresses his belief that the uric acid is driven out of the blood by high acidity and precipitated in the joints, and that such precipitation and non-destruction of the uric acid afford the true explanation of the effects of the inflammation and fever of rheumatism. He maintains that owing to the rise of temperature the acidity is greater and the precipitation of uric acid, in the joints is, therefore, more complete in rheumatism than in gout. Other causes that raise the acidity and therefore predispose to an attack of rheumatism are chilling and suppressed perspiration, ingestion of acids and acid-forming foods, and tonsillitis or other local inflammation. He says that high arterial tension at the end of an attack points to an excess of uric acid in the blood, and anything that raises the acidity at this time will cause a relapse.

The second London post-graduate course commenced recently, and like the last will consist of about seventy lectures and demonstrations at the leading special hospitals. To the list of the hospitals which were used in the last course there must be added Bethlehem Hospital, the best known lunatic asylum in London, and one of the Poor-law Infirmaries, where it has been arranged that Mr. Hutchinson will give a series of demonstrations which no doubt will constitute a most valuable addition to the course. I suppose these lectures have not become very widely known yet, for only twenty-nine men joined for the last course, a very small number considering the amount of instruction and material offered to the class.

The Charing Cross Hospital Medical School has been extensively enlarged and will be well equipped now in the way of laboratories and lecture theatres. It is not nine years since the school buildings were erected, and the necessity for such extensive alterations in so short a time speaks well for the prosperity of the school. Nearly all the metropolitan schools, however, in the last ten or twelve years have had to double their accommodations.

## NEW YORK.

*To the Editor of* THE MEDICAL NEWS,

SIR: The rapid advances in the surgery of the brain which are now being made are but the natural outcome of the elaborate and painstaking experiments which have been made by Ferrier and others in endeavoring to penetrate the darkness and solve the mysteries of this wonderful region of the human anatomy. Dr. E. D. Fisher recently added his mite to the physiology of this subject, by presenting to the New York Academy of Medicine some of the results of a series of experiments which he had made upon animals to determine the functions of the cerebellum. He trephined several days prior to the removal of portions of the cerebellum, in order to eliminate errors which might arise from the irritation caused by the trephining itself. He found that superficial lesions did not cause any symptoms, and that when these lesions were sufficiently extensive to cause incoördination, the animals apparently tried to overcome these disordered movements. He exhibited a monkey which dragged the leg on the side corresponding to the lesion, and which held the arm most of the time in a semi-flexed position. The experiments seemed to prove that there was a peculiar cerebellar incoördination.

Dr. W. Gilman Thompson said that he had witnessed many of the experiments, and had sometimes observed extraordinary movements in animals, following simple trephining. One of the great stumbling-blocks in the study of cerebral localization is the way in which recovery takes place after serious injury to the brain; and this is well shown in the present series of experiments. Dr. Thompson said that three varieties of coördination are to be distinguished, viz.: 1. A lower form, controlled by the spinal cord; 2. A higher one, controlled by the cerebellum; and, 3. A still higher variety, due to education, and controlled by certain parts of the cerebral hemispheres, which have not yet been definitely located. When one of these three divisions is injured, it is possible for the other parts of either the brain or cord to be educated to do the work of the injured portion. This does not seem surprising, when we remember that children have to learn first the simple movements of coördination, and proceed gradually to the most complicated ones. Recovery after injury takes place in the same order, beginning with the simpler movements.

In a discussion on the "Complications of Laparotomy," at a meeting of the Clinical Society of the Post-graduate School, the question of the propriety of using drainage-tubes received a good deal of attention. Dr. A. Palmer Dudley was most emphatic in denouncing their use after laparotomy unless pus is found in the abdominal cavity. He thinks the dangers of the use of the drainage-tube are tenfold greater than those arising from an ordinary oozing of blood into the abdominal cavity, and that the tube is not necessary to show the occurrence of a secondary hæmorrhage, as this is more quickly indicated by the condition of the patient. Neither could Dr. Robert T. Morris see any necessity for the use of drainage-tubes, for Nature has provided the large intestine as an efficient drainage-tube, which runs directly through the middle of the "cesspool." He said that the ordinary drainage-tube requires careful and skilled attention; but by the administration of a hydragogue cathartic, Nature's drainage-tube can be made to remove rapidly, by osmosis, the noxious material. Certain German experimenters have shown that large quantities of fermenting liquids may be absorbed by the great lymph-sac—the peritoneum—which are eliminated by the kidneys and intestinal glands. Dr. Charles C. Lee expressed the general opinion of those present when he said that drainage-tubes are oc-

casionally extremely useful, if not indispensable, but that the progress of the profession in laparotomy has shown that they are rarely necessary, and certainly did harm if indiscriminately or extensively used. Dr. J. R. Nilsen called attention to the difficulty sometimes experienced in finding the linea alba, owing to its deviating an inch or more from the median line; and he advised making a careful examination with reference to this point, prior to the operation. When dealing with intestinal adhesions, which do not yield to gentle manipulation, his rule is to enlarge the incision and to work by sight. This, he said, greatly simplifies the operation and yields better results.

A paper by Dr. C. L. Dana, entitled "A Study of Alcoholism in the Bellevue Cells," excited a general discussion among the alumni of Bellevue Hospital, which was also participated in by several neurologists and alienists. The paper stated that, annually, three thousand cases were confined in the "Bellevue cells," and that three-fourths of this number were males varying in age from twelve to seventy years. Fully one-half of these unfortunates are of Irish birth, and the preponderance of Irish parentage among the native-born is very great. These facts, he thinks, are especially serious in view of the well-known fecundity of the Irish race. The period when delirium develops is usually about twenty-four hours after admission, and is due to the withdrawal of the accustomed stimulant and to general starvation of the nervous tissues. It usually lasts only a day or two; but when it continues for two or three days, about five per cent. of the cases die of exhaustion. A glance at the annual number of deaths for the past four years shows that it has increased nearly seventy per cent., while the annual number of cases has only increased from three thousand to thirty-six hundred. The great mortality this year is partly due to the epidemic of influenza, and its complicating pneumonia. Appalling as these figures are, they could be much improved if we could prevent the sale of cheap liquor and abolish the "treating" and credit system. Cheap whiskey is freshly distilled, and contains much amylic alcohol, which is eight times more poisonous than ethylic alcohol.

Dr. Matthew D. Field then read a paper on "Inebriety." He believes that the present treatment of inebriates is radically wrong, and that dipsomania is a physical disease; hence the treatment, to be successful, should be in conformity with this view. He incidentally touched upon a social problem of much interest when he referred to the effect of poor ventilation in predisposing to drunkenness, and he cited, in support of this statement, the experience in England with a certain class of trades-people who were notoriously intemperate until their lodgings and workshops were put in a more healthy condition, when drunkenness was notably decreased. Imprisonment of the inebriate does not cure him, as is well shown in the case of a certain woman who was sent to the workhouse twenty-eight times in twenty-five months. In discussing this paper, Dr. J. West Roosevelt thought that the increased annual number of cases of alcoholism can be explained by the more than proportionate increase in the population. It is, he said, very difficult to regulate the quality of liquor sold, although there is much need for reform in this direction. He has seen many cases of alcoholism, especially the febrile ones, which simulated poisoning by the essential oils.

Mr. Clark Bell, President of the Medico-legal Society, in discussion, said that owing to the peculiar laws with reference to drunkenness in Boston, that city has the largest number of criminals in proportion to its population of any city in the world; and as a similar state of affairs exists in New York, it partially accounts for the large number of cases cited by Dr. Dana. At the present time, both the medical and the legal professions are pretty generally agreed that there is a disease called inebriety, which causes one to lose control of his conduct.

Dr. F. H. Ingram reported a collection of statistics which embraced information gathered from every nation. These statistics show, among other things, that apple brandy, or "Jersey lightning," is the most potent of all alcoholic beverages in producing mental derangement; next in order comes hard cider; then brandy and whiskey; then white wines; and, lastly, the red wines.

## CURIOSITIES OF HOMŒOPATHIC PHARMACY.

*To the Editor of* THE MEDICAL NEWS,

SIR: Having recently obtained a copy of the circular and price-list issued by a dealer in high potencies residing in New York City, I desire to impart to the profession some of the information therein contained. This document is a printed pamphlet of thirty-two pages, and is entitled "Catalogue of Morbific Products, Nosodes, and other Remedies in High Potencies." These remedies are for sale by Samuel Swan, M.D., at No. 13 W. 38th Street, N. Y.

Glancing over the catalogue, we were struck, on page 16, by one article of the materia medica kept for sale, and called "Luna, or Moonlight." How in the world is this obtained, and what are its properties? Astronomers have long disputed whether the rays of the moon possessed any heating power, but why need they any longer be in doubt, when moonlight may be bought by the vial-full, and can be seen and tasted by ordinary mortals.

On page 20 we find three species of pediculi on the list of remedies to be dispensed, namely, the pediculis pubis, pediculis capitis, and pediculis corporis. These minute insects we only expect to find on uncleanly persons, and the idea of swallowing them, or a preparation made from them, no matter how dilute, is rather startling to the uninitiated. The pediculis corporis, by the way, has a note in brackets stating that this special lot came from Boston. The inquiry, then, arises in our minds, Why is this? Are the Boston pediculi more voracious, or of larger size than those of other cities? What is the cause of their superiority? Unfortunately, the vender of these delectable preparations does not condescend to inform us upon these points, so we are compelled to remain in ignorance.

On page 1 we find on the list the name "acarus scabies" translated "lice insect." As the acarus mentioned is the one found in the disease known in common parlance as the itch, and is not the pediculis, there seems to be a (H)itch in the Latin; but such trifles need never daunt the swallower of preparations like these.

There is on the first page one familiar remedy, viz.:

adeps anserina—goose-grease. How well do we remember the vile odor of this substance when applied externally to the neck in our youthful days. Never did we imagine we should see it prepared in "high potencies," and given in granules.

On page 15, "lachryma filia" are defined as "tears from a young girl in great grief and suffering." In ancient Rome small crystal or glass flasks have been found called lachrymalia, which are said to have contained the tears of mourners. Were these lachryma filia obtained from such sources, or are they the tears of some modern damsel; mourning, perhaps, like Miss Flora McFlimsey, because she had nothing to wear, or in utter desperation from the lack of a spring bonnet; or, it may be, crossed in hopeless love?

The explanation of "flavus irides," on page 11, is "yellow ray of the spectrum." Here is bottled sunshine with a vengeance! Photographers have long desired to fix the fleeting rays of the sun, and to make their pictures glow with the colors of nature, and here we have the rays prepared and fit for internal medication. Would that we knew the secret of this preparation! One thing is certain, if we *must* swallow globules it would be much pleasanter to think of swallowing bottled sunshine than a preparation of pediculis corporis, even though it came from Boston.

On page 6 we find "carbunculus on neck (Swan); very severe." We are not told whether the carbuncle was very severe upon the swan, or whether the pus obtained from it is very severe upon the unlucky patient who takes it—perhaps both statements may be true.

"Fel gryllus Americana" (page 11), or "Brazilian cricket," is said to be beneficial for "suppression of urine with or without pain," and this important fact is also stated, that "a boy who had chills and fever swallowed a live cricket, and never had a chill afterward."

On the first page several dainties are provided for lovers of preparations of the high potencies. One is "adenia," or "glands from a person suffering from Hodgkin's disease," a little further on is the "ailanthus bug," an insect found on the odorous though unfragrant ailanthus tree. On the same page "albuminuria" is defined as "renal albumin," or, in other words, the putrid and disgusting products of the decomposition of the human body, as found in diseased urine.

On page 2, "anthracin" is said to be "pus from a carbuncle." On page 4 is "burboin syphilitica," or "pus from syphilitic buboes." On page 5 are a number of preparations of "calcarea" from different parts of the body, as "calcarea renalis," or "stone of the kidney," and "calcarea from the lungs and bladder." On page 6 we have three preparations to choose from, made from different varieties of catarrh, namely: "catarrh of the intestines," "catarrh of the bladder," and "catarrh of the nose."

Page 7 gives us a little hope that we may not be called upon to swallow any more such vile compounds, for there we find "cerulia irides" defined as "blue ray of the spectrum," but, as if to dash our newly found hopes to the ground, immediately beneath is given a preparation made from the "chancre of syphilis."

Page 8 gives a remedy for constipation in newly born infants, viz., "colostrom, found in their intestines previous to and after birth." Page 9 gives preparations of two varieties of poisonous snakes, viz., crotalus horridus (rattlesnake) and crotalus cascavella. On the same page is "crusta lactea," commonly called "milk crust," and found on the heads of sickly or uncleanly babies. Further down, on the same page, are preparations of diabetes mellitus, and also of the serum found in cases of dropsy.

Pages 10 and 11 give us preparations entitled "electricitas, or electricity," and "galvanismus, or galvanism." It has been said of our countryman Franklin that he brought down lightning from heaven for the service of man, but our homœopathic brethren can do more, they can make it edible and drinkable for ordinary mortals.

But time fails me, and I can only enumerate a few of the choice morsels obtainable in the high potencies: "hippozinine," a preparation from glanders; "lyssin," from hydrophobic patients; "osteo-necrosis," from necrosed or dead bone; "pus from abscess of rectum," from "caries of heel," and from septic abscess.

Page 22 gives us "rubrum irides, or red ray of the spectrum," completing the list of rays of light used in the manufacture of these globules. On page 23 we find four varieties of cancer served up to us, viz., cancer of the breast, uterus, bowel, and face.

Page 25 seems to show that Dr. Brown-Séquard's so-called Elixir has been anticipated, for we there find two preparations: one "testiculo gallinæ," or "testicles of the fowl," and "testiculo muliebris sinistra," from the left ovary of a woman.

Many other preparations might be given, but I will conclude by giving an extract from the note after page 30. Dr. Swan says that the plan of sending grafts has been adopted, viz., "If a graft is put into a vial filled with unmedicated pellets, and corked, the whole mass will be medicated in half an hour." Further down, on the same page, he says: "When a vial of medicine is nearly emptied, fill it with unmedicated pellets, and you will not have to purchase the remedy a second time."

"Can the farce of humbug further go?"

ROBERT REYBURN, M.D.,
Professor of Physiology and Clinical Surgery in the Medical Department of Howard University.
WASHINGTON, D. C.

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## GYNECOLOGICAL ELECTRO-THERAPEUTICS.

*To the Editor of* THE MEDICAL NEWS,

SIR: In the issue of THE MEDICAL NEWS for May 10, 1890, I find an article on "Gynecological Electro-therapeutics," by Horatio R. Bigelow, M.D., in which he refers extensively to an article of my own, on "Electricity in Chronic Pelvic Inflammatory Diseases," which appeared in your issue of March 22, 1890. The vital points I made in my paper were that electricity never actually cured chronic pelvic troubles, and that the most it could do was to relieve the symptoms temporarily and leave the diseased tissue, which at some future time would again develop acute symptoms. My object in again burdening your readers with this subject is to call attention to how thoroughly Dr. Bigelow corroborates my statements as made on that and other occasions, although his paper is an apparent criticism of my position; or, at least was meant to be such. Dr. Bigelow

has now been in constant attendance at Apostoli's clinic for about one year, and in speaking he gives not only his own opinions, gained from his personal observation, but reflects the opinions of Apostoli—opinions based on an experience in gynecological electro-therapeutics second to none in the world. Dr. Bigelow says, "I have *never* seen a pyo- or a hydro-salpinx anatomically cured. There are many of these cases which should never be electrically handled. Dr. Apostoli, my master, has never claimed in his writings that he cured these cases anatomically." And again, "some conditions of salpingo-ovaritis may be *symptomatically* cured."

This is exactly what I have claimed, and nothing more.

Now, in spite of this personal experience, Dr. Bigelow, in speaking of those gentlemen who claim to cure these diseases *anatomically* with electricity, thinks that "the word of any professional brother who has carved out for himself recognized standing must be accepted without the shadow of a doubt." He further observes, "If Dr. Goelet says that he has dissipated these adhesions, I cannot conceive why we should not accept his statement, although clothed in poetic language. The science must go forward or backward, it cannot stand still, and improvements will be made upon Apostoli's plan by his disciples as experience and knowledge increase, so that you will have more extended results in America than here. . . . I have not seen a case of chronic adhesions absolutely cured anatomically, but I am quite ready to believe that it is possible." In this, as in many other articles lately written, those of us who do not believe that electricity will *cure* these diseases, are reproached for throwing discredit on the honesty of our opponents. This is not true, however, and is simply begging the whole question. We give all due credit to their honesty and know full well that they believe what they say about this remedy and the results of its use. There is room for a great difference of honest opinions on most medical subjects, and this is no exception to the rule. The difficulties of accurate diagnosis in pelvic inflammatory diseases are so great that we claim little in that direction which we do not grant to our opponents. It is here that we think these gentlemen are at fault and where they go astray, and this could easily be proven by numbers of individual cases were it necessary. Now, if Apostoli and Bigelow fail to obtain the cures claimed by their disciples and I also fail after repeated and prolonged trials, is it reasonable to suppose that I will accept without question the statement of other men when they say they have obtained these cures, especially when I go to some of these same men and ask to see a case cured by them, and when after several years of constant observation they fail to show me a single cure, although in the meantime they are constantly talking of the cures they have made? I repeat, are we not bound under these circumstances, in the interest of science, to refuse to accept their results unless accompanied with the most convincing proof? I can say, with Dr. Bigelow, "I have not seen a case of chronic adhesions absolutely cured anatomically," but there he and I must part company on this road, for in spite of all the claims made by others, I am thoroughly convinced that it is impossible to cure them with electricity.

J. M. BALDY, M.D.

330 SOUTH SEVENTEENTH ST., PHILADELPHIA.

## SHORT-WEIGHT QUININE CAPSULES.

*To the Editor of* THE MEDICAL NEWS,

SIR: In your issue of May 3d there appeared a letter from Professor V. C. Vaughan, of Ann Arbor, on "Short-weight Quinine Capsules." In this letter a detailed description of our tin boxes of 100 filled capsules, labels, etc., is given, together with an analysis of contents of four boxes (of formula No. 106: Quinine sulphate 3 grs., capsicum ⅓ gr.), to which *his attention had been called*, showing shortages in the capsules. Dr. Vaughan's analyses indicate that boxes 1 and 2 contained no capsicum, a defect not noticeable in boxes 3 and 4. Our tins of 100 Elastic Filled Capsules are sealed with tinfoil before leaving our house, and we have direct knowledge that the first two boxes which were submitted to Dr. Vaughan were *not intact*. The Detroit jobber to whom these goods were sold finds it inconvenient to furnish us with the name of the purchaser, and Dr. Vaughan is disinclined to give any information, except such as relates directly to his analysis.

As the formula in question is not frequently called for it was fortunately a matter of no difficulty for us to trace the date of manufacture of this particular order, which was prepared during the rush of the grippe season by additional help specially employed for such work. As it happened, the identical employé who filled the goods in question was soon after detected in shortage work. Whether this shortage work was due to intentional carelessness is a question we reserve for discussion at a later date and in another place. But we do not reserve mention of the use which has been made of Professor Vaughan's letter. Hardly had it appeared in your columns before our Chicago agent apprised us of the fact that one of our competitors located in Detroit was distributing with liberal hand a circular containing an extract of the article above mentioned, and was using it against us. The fact bears its own comment. If they believe that this sort of tactics will do them any good they are welcome to the profit and the honor that are in it. Let them make the most of the above-outlined occurrence. We content ourselves with calling attention to their proceeding, and we give them fair warning that we shall hold them strictly accountable for any inference they may attempt to cast on our goods, outside of the capital they can make out of the aforesaid occurrence, which fortunately we are able to trace and account for.

We can state that our trade on filled capsules has been increasing most rapidly, especially so since we have recently placed our capsules directly before the medical profession through our representatives or by means of direct correspondence, and this has doubtlessly raised the ire of our competitors, who no doubt think that they alone should have the undivided attention of the profession. We make over 125 standard formulæ of filled capsules. Of these there are only seven which contain medicine in dry form. The rest, containing fluids, are filled by a machine invented by us and especially designed to secure as absolute exactness in filling as is possible. However, our professional friends require no assurance that our goods are all that is claimed for them and we are content to leave our friends to determine in the present criticism just where the shoe pinches.

Respectfully,

DETROIT, May 28, 1890. THE MERZ CAPSULE CO.

### A DEFENCE.

*To the Editor of* THE MEDICAL NEWS,

SIR: Your representative has called our attention to Dr. Austin Flint's letter, published in the issue of your journal of May 3d, and, although our intention was to pay no attention whatever to such plausibly malicious attacks, based on private motives, we address you at the special urgence of a number of our friends in the medical profession, and trust you will kindly give space to our reply.

In referring to members of the alumni of Bellevue Hospital and other medical colleges, it should be clearly understood that each physician referred to by us as endorsing "Vin Mariani" has given us full authority to do so. Upward of 7000 letters bearing physicians' signatures can be seen any time at this office, having been purposely and kindly given us to aid in the further introduction of "Vin Mariani," it being well known that such advertisements are confined exclusively to those channels recognized and approved of by the severest code of ethics, and a method which we are justly proud to boast has, during thirty years, won for "Vin Mariani" the most thorough approbation and appreciation of the entire medical profession.

We are to infer from Dr. Austin Flint's letter that, owing to our recent publication, he intends to discontinue the use of "Vin Mariani;" this he is, of course, at perfect liberty to do; but we positively assert, both himself and his eminent father have for years ordered "Vin Mariani" for use in their own family, as well as very largely for their patients, and this we can and will attest by prescriptions and letters bearing the name, "Austin Flint, M.D.," which we hold subject to inspection. We doubt that any reputable practitioner would, from a personal motive, withhold from his patient a preparation which he has prescribed in the past, and which practical testing for years has proved to be a valuable therapeutic agent, and we may be permitted to state that no recognized medicinal preparation has received stronger endorsement at the hands of the medical profession than "Vin Mariani." Incidentally we call attention to an article published on the same date as was Dr. Flint's letter, in *The Journal of the American Medical Association*, by Dr. Charles E. Sajous, in which "Vin Mariani" receives the highest commendation (see page 645).

Respectfully yours,

MARIANI & CO.

## NEWS ITEMS.

***Myopia of Schools.***—Dr. Kirchner, of Berlin, has investigated the causation of myopia as shown by the pupils at two of the largest gymnasia in the city. One of these schools is old, badly constructed, and poorly lighted. The other is a new building designed with special reference to good illumination. Dr. Kirchner found that, at the old school, short-sightedness increased progressively from the lowest class up to the highest, whereas in the new building it did not begin to increase until the middle classes were reached.

***Medicine in Siam.***—A new medical mission has been opened at Rathboree, Siam, the third city in importance in the kingdom. The former Prime Minister has presented a large brick station, well built and very conveniently located.

***The Chair of History of Medicine.***—Dr. A. B. Lyman has been elected to fill the newly created Chair of History of Medicine in the University School of Medicine at Baltimore.

***Medical Education of Women in Austria.***—A petition has recently been presented to the Austrian House of Deputies, asking for the admission of women to the philosophical and medical faculties of the universities of the empire. It is stated that the petition was signed by 4812 people, including many leading representatives of literature, art, and science.

***Progress of Cremation.***—The new cremation furnace of Paris at the cemetery of Père la Chaise, has been used 735 times in its first four months of operation. Of this number, 35 only were at the request of surviving relatives; by far the greater number were unclaimed bodies of those who had been drowned, and stillborn children.

The Senate of Hamburg has authorized the construction of a furnace, with columbarium, at a cost of $19,000. The funds will be collected by a local society which has been in existence for several years and working quietly in favor of cremation.

The cost of cremation appears to have undergone a marked reduction. The Paris correspondent of the *British Medical Journal* writes that at the new crematorium, above referred to, the expense for each firing is placed at three francs instead of thirty-five francs as in the case of the older establishment. Coke is the fuel employed at the new furnace.

***Medical Missions.***—The Edinburgh Medical Missionary Society will celebrate its jubilee anniversary in 1891 All of its orginal board of directors save one are dead. At home it controls the Livingstone Dispensary and Training Institution, caring for 10,000 patients yearly. Abroad, it has branch institutions or missions in India and Palestine, with more than sixty physicians and surgeons. In 1860 there were only 14 qualified medical men in the foreign field; now there are 280, of whom 37 are female physicians. In China there are 82 medical missionaries, a majority of whom are from the United States. There are mission hospitals and dispensaries in several of the principal cities, and they get a considerable portion of their financial support from the better classes of the Chinese, who highly appreciate the work, or at least the medical side of it. The Hangchow Mission, in its last annual report, shows that it ministered to 9,000 patients with 24,000 treatments; the number of in-patients was 550.

***Artificial Musk.***—The *Journal of Pharmacy* of Alsace-Lorraine states that a very powerful musk has been made artificially by a distillation of toluol with the chlorides of aluminium and butyl. So far as the use of musk in perfumery is concerned, it is stated that the natural article will sooner or later be supplanted by the artificial. As to the medical employment of musk, at the present time it is very restricted, and, indeed, hardly exists. If the

natural musk shall become a cheaper and less adulterated commodity, it may again be used in medicine, especially in adynamic diphtheria, for which Dr. Jacobi has found it sometimes valuable.

***Bayonne, New Jersey, Hospital.***—A new hospital has been established at Bayonne, New Jersey, having a capacity of forty beds. It has been furnished with all the new and most approved appliances that can be made available in a small hospital.

***Physiology at Vienna.***—After the retirement of Professor Ernst von Brücke from the chair of physiology in the University of Vienna, which will take place at the end of the present scholastic year, there will be two professorships provided for this branch of instruction. According to the *British Medical Journal*, this new departure has been rendered necessary by the fact that the classes have become too large to be satisfactorily managed by one professor.

***Medical Progress in the East.***—A medical college has been established at Antananarivo, the capital of Madagascar, in connection with a Norwegian missionary enterprise. There have already been graduated a number of well-trained native physicians, whose lives will be spent in distant provinces which European missionaries have not yet been able to reach. A new hospital, at the college, will soon be added to the advantages of the Norwegian station. The Society of Friends, of England, has, at the same city, an extensive medical mission under the supervision of Drs. Fenn and Moss.

***"Doctor Pascal."***—It is stated that the last book of Emile Zola, entitled *Doctor Pascal*, was in part founded on the unfortunate family history of the celebrated physiologist, Claude Bernard, who said of himself that he was "the martyr of marriage."

***The Delta Sign in Prescribing.***—The *Western Druggist* notices a tendency on the part of certain physicians to employ the Greek delta as a symbol for the drachm in prescriptions.

***Obituary.*** — Dr. Samuel Rush Haven, formerly of Chicago, died, in his sixty-fourth year, at Joliet, at the house of his brother, Professor Dwight Haven, on May 5th. He was eminent in surgery, and in the late war was staff surgeon to Generals Hancock and Grant. He was a native of Sheridan, N. Y., and was an alumnus of the Rush Medical College, from which he was graduated in 1850.

### *Corrigendum.*

ASEPTIC MIDWIFERY.

In THE MEDICAL NEWS, May 24th, page 577, it was incorrectly reported that Dr. T. B. Greenley advocates antiseptic douches during and after labor.. Dr. Greenley discountenances the use of vaginal douches in labor and during the puerperium.

### OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JUNE 3 TO JUNE 9, 1890.

DIETZ, WILLIAM D., *Captain and Assistant Surgeon.*—Is granted leave of absence for one month.—Par. 4, *S. O. 131, A. G. O., Washington, D. C.*, June 5, 1890.

By direction of the Secretary of War, WILLIAM D. DIETZ, *Captain and Assistant Surgeon*, having completed at New York City the duties assigned him in Special Order No. 29, April 30, 1890, Division of the Pacific, will return to his station in that Division.—Par. 5, *S. O. 129, A. G. O., Washington, D. C.*, June 3, 1890.

By direction of the Acting Secretary of War, leave of absence for seven days, to take effect upon the final adjournment of the Army Medical Examining Board, is granted HENRY MCELDERRY, *Major and Surgeon*—Par. 7, *S. O. 127, A. G. O., Washington, D. C.*, May 31, 1890.

By direction of the Secretary of War, leave of absence for six months, on surgeon's certificate of disability, is granted PETER J. A. CLEARY, *Major and Surgeon.*—Par. 4, *S. O. 127, A. G. O., Washington, D. C.*, May 31, 1890.

With the approval of the Secretary of War, leave of absence for ten days is granted CHARLES WILLCOX, *First Lieutenant and Assistant Surgeon.*—Par. 16, *S. O. 126, A. G. O., Washington, D. C.*, May 29, 1890.

### OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING JUNE 7, 1890.

KITE, I. W., *Passed Assistant Surgeon.*—Detached from Naval Hospital, Pensacola, and ordered to Hospital, New York.

CRAIG, THOMAS C., *Passed Assistant Surgeon.*—Detached from Naval Hospital, New York, and ordered to the U. S. S. "Vesuvius."

ANDERSON, FRANK, *Passed Assistant Surgeon.*—Granted leave of absence for the month of June.

OGDEN, F. N., *Assistant Surgeon.*—Ordered for examination preliminary to promotion.

WHITE, S. S., *Assistant Surgeon.*—Ordered for examination preliminary to promotion.

BEYER, HENRY G., *Passed Assistant Surgeon.*—Ordered to duty on the U. S. S. "Yantic."

SIMONS, MANLY H., *Surgeon.*—Detached from Widow's Island Hospital, and placed on waiting orders.

HAWKE, J. A., *Surgeon.*—Ordered to Widow's Island Hospital and attending officers of Navy and Marine Corps at Portsmouth, N. H.

### OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE-HOSPITAL SERVICE, FROM MAY 11 TO MAY 31, 1890.

PURVIANCE, GEORGE, *Surgeon.*—Detailed as Chairman of Board of Examiners, May 31, 1890.

LONG, W. H., *Surgeon.*—Granted leave of absence for three days, May 19, 1890.

GODFREY, JOHN, *Surgeon.*—Detailed as a member of Board of Examiners, May 31, 1890.

FAIRFAX, IRWIN, *Surgeon.*—Detailed as Recorder of Board of Examiners, May 31, 1890.

CARTER, H. R., *Passed Assistant Surgeon.*—Ordered to examination for promotion, May 31, 1890.

BANKS, C. E., *Passed Assistant Surgeon.*—To proceed to Boston, Mass., on special duty, May 2, 1890.

PECKHAM, C. T., *Passed Assistant Surgeon.*—Granted leave of absence for eight days, May 14, 1890.

AMES, R. P. M., *Passed Assistant Surgeon.*—To proceed to Memphis, Tenn., for temporary duty, May 14, 1890. To proceed to Gulf Quarantine Station for temporary duty, May 31, 1890.

PERRY, T. B., *Assistant Surgeon.*—Ordered to examination for promotion, May 31, 1890.

CONDICT, A. W., *Assistant Surgeon.*—Granted leave of absence for twenty-two days, May 24, 1890.

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.*

*Local papers containing reports or news items should be marked.*

*Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.*

*All communications relating to the editorial department of the* NEWS *should be addressed to No. 1004 Walnut Street, Philadelphia.*